Economic Impacts of Regional Approaches to Rural Development:

Initial Evidence on the Delta Regional Authority

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Abstract

This study assesses the initial economic outcomes of the Delta Regional Authority (DRA), which began funding rural development projects in the Mississippi Delta region in 2002. The study focuses on non-metropolitan DRA counties and similar counties elsewhere in the Mississippi Delta region and the southeast, using a quasi-experimental approach that combines matching methods, double difference and switching regression estimation. We find that per capita income and transfer payments grew more rapidly in DRA counties than similar non-DRA counties, and that these impacts are larger in counties in which DRA spending was larger. Each additional dollar of DRA spending per capita is associated with an increase of $15 in personal income per capita between 2002 and 2007, including an increase of $8 in earnings (primarily in the health care and social services sector) and $5 in transfer payments. The increase in transfer payments is mainly due to increased medical transfer payments. We also find that the number of hospital beds per capita increased more in counties where DRA spending per capita was greater. These findings suggest that investments supported by the DRA in improved medical facilities are promoting additional health sector earnings and medical transfer payments.

1. Introduction

Regional economic development approaches are becoming increasingly popular. More than 30 years after the Appalachian Regional Commission (ARC) was established in 1965, Congress authorized the Denali Commission in 1998 and the Delta Regional Authority (DRA) in 2000 to promote rural development in Alaska and the Mississippi Delta region, respectively. Since 2000, four additional regional rural development commissions have been authorized, and start-up funds have recently been appropriated for two of these. The popularity of this approach is due in part to favorable evaluations of earlier interventions such as the ARC (e.g, Isserman and Rephann 1995). To date, however, there have been no published studies assessing the initial outcomes of the DRA or the Denali Commission, although such studies could be very helpful in guiding further policy decisions about such programs. This study addresses this information need, investigating initial outcomes of the DRA on rural development outcomes in the Mississippi Delta region.

The DRA is a partnership between the Federal Government and eight Delta states, targeting 252 economically distressed counties. It initiated operations in 2001 and began funding projects in 2002. Between 2002 and 2008 the DRA invested $63 million in projects related to basic public infrastructure, business development, transportation infrastructure, job training and employment related education. The program reports that it leveraged an additional $296 million in other public investment and $1.4 billion in private investment during this period.

In this study we investigate the initial outcomes (during 2002 to 2007) of the DRA's investments in nonmetropolitan DRA counties using a quasi-experimental approach that combines matching methods, double difference and triple difference estimation, and switching regression models to reduce the confounding influences of both observable and unobservable factors and allow for heterogeneous impacts. The outcome variables used in the assessment include county level changes in personal income per capita and its components, employment per capita and its components, and population. The covariates used in the matching and regression analysis include prior levels of many relevant socioeconomic and demographic characteristics. We investigate the robustness of our findings to alternative methods, model specifications, and different baseline and ending dates of the analysis. In general, our findings are quite robust to these variations.

This study is of an exploratory nature, seeking to identify whether significant differences in outcomes can be measured for an economic development program as small as the DRA after only five years of implementation. It is not an impact evaluation, but rather a test of whether available data and econometric methods can discern potential impacts and help to illuminate the possible mechanisms of impact. If some initial impacts are evident, this may point to useful avenues of further research to better understand these impacts, and to rule out alternative explanations. If no significant impacts are evident, it does not mean that the program had no impact; rather it may simply mean that the impacts that have occurred are not measurable with the data and methods available, given the relatively small size of the initial DRA funding levels, or that a longer time must elapse for measurable impacts to occur.

The next section provides background on the DRA. The third section presents the methods and data used in the analysis. The results are presented and discussed in the fourth section, and the fifth section concludes.

2. The Delta Regional Authority

The Delta Regional Authority (DRA) was authorized in December 2000 as part of the FY2001 Omnibus Appropriations package (PL106-554) and has been reauthorized by the 2002 and 2008 Farm Acts. Although funding was appropriated for the DRA, the funds appropriated have been well below the level authorized ($30 million per year) (Table 1). Since its inception, DRA expenditures have been much smaller than those of other development organizations such as the Economic Development Administration, the ARC, or the Denali Commission.

The DRA is a Federal-State partnership involving eight States (Illinois, Missouri, Kentucky, Tennessee, Arkansas, Mississippi, Louisiana, and Alabama). It is led by a Federal Co-Chair and the Governors of the participating States. Within these States, 252 counties and parishes are currently eligible for the program (Map 1). The population of this region was 9.5 million in 2000. These counties were identified on the basis of economic distress, considering per capita income, poverty, unemployment and other indicators.

The DRA authorizing legislation requires that at least 75 percent of the DRA funds be used to serve the needs of distressed counties. DRA project funding has far exceeded this threshold, with 94 percent of project funding during 2002 to 2008 going to distressed counties (DRA 2009). This is because almost all of the eligible DRA counties are distressed. For example, an Economic Research Service (ERS) analysis of the 219 counties within the original Lower Mississippi Delta region (excluding Alabama) found that the large majority of these counties were nonmetropolitan and that most were persistent poverty counties (Reeder and Calhoun 2002). The average poverty rate of this region in 1999 was 18.8 percent compared with 14.6 percent for non-metro counties nationwide. The non-metro Delta also had higher than average unemployment rates, a high concentration of African Americans (31 percent), per capita income well below the U.S. average, and a relatively low rate of population growth, particularly among its poorest counties. Thus, this region was economically distressed, no matter which measure was used.

The DRA is authorized to provide grants to States and public and non-profit entities for development projects, with the following order of priority: 1) basic public infrastructure in distressed or isolated areas; 2) transportation infrastructure facilitating regional economic development; 3) business development, with emphasis on entrepreneurship; and 4) job training or employment-related education. At least 50 percent of project grant funds are required to be for transportation and basic public infrastructure projects. From 2002 to 2008, 76 percent of DRA project funds were invested in these priority projects (Ibid.). The DRA began funding projects in 2002, although major project implementation did not begin until 2003.

The DRA recognized from the outset that its ability to achieve improvements in outcomes would be limited by its modest budget and staff resources. The model pursued was to concentrate on developing the assets needed to sustain long term growth in selected critical mass communities (i.e., communities and activities that have the necessary elements in place to successfully promote economic growth) by coordinating efforts of multiple organizations and leveraging additional public and private investments.

During its first seven years of operation (2002-2008), the DRA invested $63.2 million in 435 projects, operating in 166 of the 240 counties eligible during this period (DRA 2009). The DRA reports that this investment leveraged $296.2 million in other public funds ($4.69 in additional public funds per $1 invested) and $1,442.3 million in private investment. One important factor contributing to the potential leverage of the DRA is the fact that DRA funds can be treated as local counterpart funds for Federal matching grant programs, thus enabling investments that might not otherwise be feasible.[1] The most common type of investment supported by DRA funds was investment in water and/or sewer systems; these accounted for 29 percent of DRA project funds invested during 2002 to 2008 (Figure 1). Following this were investments in roads (12 percent of project funds), industrial parks (9 percent), education and training (8 percent), port facilities (8 percent), medical facilities (7 percent), and business development (5 percent).

3. Methods and Data

In this section we discuss the analytical methods, study population, variables and specifications used to estimate initial outcomes of the DRA, and the data and data sources used.

Analytical methods

To assess the outcomes of a program for program participants, one needs to estimate the counterfactual situation of what would have happened to those participants in the absence of the program. Since the counterfactual is not observed, the idea is to find other non-participating units of observation that are not affected by the program but are as similar as

[1] Pete Johnson, DRA Federal Co-Chairman, personal communication.

possible to the program participants prior to the program. This assumes that outcomes for units that were similar before the program are similar to outcomes that program participants would have experienced without the program. The best way to identify non-participants that are similar to the participants, if it is feasible, is to use randomized assignment of participants in the program, since this assures that the distributions of all characteristics of participants and non-participants are statistically indistinguishable (Heckman, et al. 1998).

Unfortunately random assignment is often not possible, as in this case. In such situations, one can use quasi-experimental matching approaches that select non-participants that are similar to participants in terms of selected observable characteristics. This matching method can reduce biases caused by differences between program participants and non-participants (or "treated" vs. "controls") in these observable characteristics; i.e., this addresses the problem of "selection on observables" (Ibid.). However, this does not assure that differences between the treated and controls in unobserved characteristics are negligible, and to the extent that such unobserved differences contribute to differences in outcomes, this could bias the results of the analysis (i.e., the problem of "selection on unobservables").

We address these problems by combining the use of matching methods with difference-in-differences (DD) estimation. The DD estimator estimates the average impact of a program on the participants (or average impact of the treatment on the treated (ATT)) as the difference between the mean outcomes for the treated and control groups after the program is implemented, minus the difference in outcomes before the program is implemented; i.e., $(EY_{T1} – EY_{C1}) – (EY_{T0} – EY_{C0})$, where EY_{T1} and EY_{T0} are the mean outcomes for the treated group in period 1 and 0, respectively (where period 1 is during or after program implementation and period 0 is before implementation), and EY_{C1} and EY_{C0} are the mean outcomes for the control group. This is equivalent to the change in mean outcome for the treatment group minus the change in mean outcome for the control group $((EY_{T1} – EY_{T0}) – (EY_{C1} – EY_{C0}))$. This estimator subtracts out the effects of any time invariant additive factors that differ between the treated and control groups and any common trends affecting both groups. Thus, as long as the differences between the two groups are time invariant, this method eliminates bias due to selection on unobservables or observables (Imbens and Wooldridge 2009).

Unfortunately, the assumption that differences between the two groups are time invariant may fail to hold in practice. For example, development programs may be attracted to locations where incomes are rising more rapidly (or more slowly) for reasons other than the program. One way to address this potential problem is to use the DD estimator for matched treatment and control groups, in which the variables used for matching are those that are expected to differ between the groups and to influence changes in outcomes over time (Ravallion 2008). This approach is similar to the conditional difference-in-differences estimator proposed by

Heckman, et al. (1998), which they found to be a promising method to address selection bias in evaluating a job training program. Smith and Todd (2005) also found that this approach substantially reduced the bias in evaluating a job training program caused by time invariant sources of cross sectional variation, and that the advantages were robust across a range of matching methods and model specifications using different subsamples of the data and different survey instruments. Isserman and Rephann (1995) used this approach to assess the impacts of the ARC, combining Mahalanobis metric matching with DD estimation of differences in growth rates of income, population and earnings between ARC and matched non-ARC counties. Ravallion and Chen (2005) also used this approach, using propensity score matching to reduce observable pre-project differences between participants and non-participants in a development project in China, and then DD estimation for the matched sample.

We use several alternative matching estimators combined with DD estimation. Propensity score matching (PSM) has been used in many studies of impacts of social programs. PSM matches participants and non-participants according to the probability of program participation (or "propensity score", denoted P(X), where X includes the observable characteristics used to predict participation). In the seminal study on this approach, Rosenbaum and Rubin (1983) proved under the assumption of "unconfoundedness" – i.e., that the outcome that would have occurred without the treatment (denoted as Y_o) is independent of treatment status (D), conditional upon X – that Y_o is also independent of treatment status conditional upon P(X), provided that $0 < P(X) < 1$.[2] Under this assumption, matching on the propensity score is sufficient to insure that the outcomes for the matched non-participant group are statistically indistinguishable from the outcomes that the participants would have experienced in the absence of the program.

We use PSM nearest neighbor matching (PSM-NN), with and without replacement. Matching with replacement allows control observations to be used as the best match for more than one treated observation; hence it tends to obtain better matches with less potential bias resulting from imperfect matches. However, use of fewer control observations results in larger standard errors and in many cases a larger mean squared error, despite less bias (Zhao 2004; Smith and Todd 2005). We also use PSM with kernel matching (PSM-KM), which estimates matching observations based on a weighted average of observations from the non-participant pool, with the weights a declining function of the distance of each observation (in terms of its propensity

[2] The assumption that $0 < P(X) < 1$ ensures that there are members of the comparison group for both treated and untreated units of observation. That is, if $P(X) = 0$ there are no treated observations for this value of X, and if $P(X) = 1$, there are no control observations. To estimate the average effect of the treatment on the treated (ATT), the assumption that $P(X) > 0$ is not necessary, since the requirement is only to find matches for each treated observation (the requirement $P(X) < 1$ is necessary in this case). If instead of ATT, the average treatment effect on the population (ATE) is to be estimated (including the potential impact of the treatment on controls), then the assumption $P(X) > 0$ is also necessary.

score) from the observation in the treatment group to be matched (Heckman, et al. 1998). Kernel PSM is able to obtain lower standard errors than NN matching, since it uses more information to construct the counterfactual observations, but this may be at a cost of increased bias (Caliendo and Kopeinig 2005). We use the Epanechnikov kernel function and a bandwidth of 0.06, which are the default options in the Stata procedure used for PSM (Leuven and Sianesi 2003).[3] To avoid observations with very high (P(X) near 1) or low (P(X) near 0) propensity scores, which will have poor matches, we impose a condition of "common support", which drops treatment observations whose estimated propensity score is higher than the maximum or less than the minimum estimated propensity score of the control group (Ibid.).

We also use the Mahalanobis metric (MM) matching estimator. The MM estimator minimizes the distance function $d_{TC} = (X_T - X_C)' \sum^{-1} (X_T - X_C)$, where X_T and X_C are vectors of matching variables for the treatment and potential control observations (considering all possible controls, and not only matched ones), and $\sum$ is the variance-covariance matrix of X_C. This estimator has been used in numerous studies of impacts of interventions in rural areas, such as the Appalachian Regional Commission (Isserman and Rephann 1995), prison construction (Glasmeier and Farrigan 2007) and rural broadband access (Stenberg, et al. 2009), among others.

There is no theorem comparable to that of Rosenbaum and Rubin (1983) providing a theoretical justification for the MM method, and it often is more biased (in terms of differences in mean values of X_T and X_C in matched samples) than PSM, especially when a large number of covariates are involved (Gu and Rosenbaum 1993; Zhao 2004). Intuitively, PSM achieves balance by implicitly giving greatest weight to matching on the variables that have significant association with the treatment assignment. MM matching attempts to achieve balance in all covariates, weighted by the inverse variance matrix of the covariates, and so may overweight variables that have little association with the treatment assignment (and hence are of little concern regarding bias), especially with a large number of covariates. Nevertheless, the MM estimator often has lower standard errors than the PSM estimator and in many cases lower mean squared error, despite being more biased (Zhao 2004).

Another advantage of the MM estimator relative to PSM is that the estimated standard errors for MM are asymptotically consistent, provided that the bias resulting from imperfect matching on covariates is corrected (Abadie and Imbens 2006).[4] To address the bias, we use the MM

[3] In general, results of PSM-KM are not very sensitive to the choice of the kernel function, as with non-parametric regression approaches (Caliendo and Kopeinig 2005; DiNardo and Tobias 2001). The choice of the bandwidth parameter appears to be more important, but involves a tradeoff between bias and variance – i.e., a high bandwidth yields a smoother density function estimation and reduced variance, but may be more biased by smoothing out underlying features of the actual function (Caliendo and Kopeinig 2005).

[4] Abadie and Imbens (2006) proved the consistency and asymptotic normality of a class of bias-corrected covariate matching estimators that includes the Mahalanobis metric as a special case (Ibid., footnote 4, p. 239).

version of the matching estimator developed by Abadie, et al. (2004), which corrects the bias in estimating the ATT using a linear least squares regression of the outcome on the covariates for the matched control observations.[5] Abadie and Imbens (2007) showed using Monte Carlo simulations that their bias corrected estimator substantially reduces bias and mean squared error compared to matching without bias adjustment and to linear and quadratic regression models. This estimator is available only for nearest neighbor matching with replacement, so we implement it for that case only.

For PSM, the estimated standard errors are not valid, both because of imperfect matching and because the estimated standard errors do not account for the fact that the propensity scores are estimated in a first stage estimation. We address the bias in one version of the PSM model (nearest neighbor with replacement) using the bias corrected estimator of Abadie, et al. (2004). In this case, we use the estimated propensity score from a first stage probit model as the single covariate in the covariate matching algorithm.[6] This reproduces the ATT estimated by the standard PSM model when no bias correction is used, although the estimated standard error is different. With the bias correction, this estimator corrects for the effects of differences in propensity scores (but not in the individual covariates) between the treated and matched control observations on the estimated counterfactual outcome.

We use bootstrapping to estimate the standard errors for all PSM estimators used (PSM-NN with replacement, with or without bias correction; PSM-NN without replacement, PSM-KM). This is standard practice among researchers to account for the fact that the propensity scores are estimated in a first stage estimation. Abadie and Imbens (2008) proved that the use of bootstrapping is not generally valid for matching estimators, and demonstrated the inconsistency of the bootstrap estimator for a specific case of nearest neighbor covariate matching (for a scalar covariate) with replacement. They argue that bootstrapping may be valid with kernel PSM estimation because the number of matches increases with sample size, but do

[5] Formally, Abadie, et al. (2004) estimate the counterfactual outcome for each treated observation i (Y_{oi}) as: $Y_{oi} = (1/\#m(i)) \sum_{k\in m(i)}\{Y_{ok} + \mu_o(X_i) - \mu_o(X_k)\}$, where m(i) is the set of matched control observations to treated observation i, #m(i) is the number of matched observations in this set, Y_{ok} is the outcome of matched control observation k (within m(i)), and $\mu_o(X)$ is the estimated linear regression function of the outcome on the covariates within the matched control group. The terms $\mu_o(X_i) - \mu_o(X_k)$ correct the estimated counterfactual outcome for differences resulting from differences in the values of the covariates between the treated (X_i) and matched control observations (X_k).

[6] We use a probit model to estimate propensity scores. Other parametric probability models, such as a logit or linear probability model, are also commonly used, as well as non-parametric probability models. Results of propensity score estimation with a binary treatment are generally not highly sensitive to the choice of probability model (Zhao 2004; Caliendo and Kopeinig 2005).

not prove this. Despite this problem, we use bootstrapping to estimate the standard errors for our PSM models due to lack of a suitable alternative.[7]

As we have seen, no matching method is clearly superior to all others in terms of both bias reduction and efficiency. Furthermore, PSM models suffer from inconsistent estimation of the standard errors. Although the MM estimator with bias correction has the advantages of being bias corrected and using asymptotically valid estimates of the standard errors, it generally has to correct for larger biases than PSM estimates, and thus can be greatly affected by the linear regression model used to correct for bias. This is an important drawback, since one of the advantages of matching methods over parametric regression methods is that they seek to avoid dependence on parametric assumptions about the relationships between the covariates and the outcome variable. Given these tradeoffs, we investigate the robustness of our conclusions to these different matching methods. To investigate how much difference is made by the bias correction, we report the results of the MM estimator and the PSM-NN estimator (without replacement in both cases) both with and without the bias correction.

Although the combination of matching with the DD estimator helps to control for confounding factors, there still could be unobserved differences between DRA and matched non-DRA counties that account for differences in their growth trends. One way to test for this concern is to test for differences in outcome trends between treatment and control groups prior to implementation of the program (Imbens and Wooldridge 2009). In their study of the impacts of the ARC, Isserman and Rephann (1995) implemented this test, evaluating the differences in trends of selected outcomes during the six years prior to enactment of the program. If the assumptions of the DD estimator are valid, and there are no program effects prior to program implementation (i.e., no effects due to anticipation of the program), the estimated pre-program differences in trends between program participants and non-participants should equal zero. We test for such pre-program differences in trends using data on the outcome indicators during the two years prior to implementation of the DRA, and find these to be statistically insignificant in most cases.

Because there are several outcomes with significant pre-program differences in trends, we also estimate changes in outcome trends from before to during the DRA implementation period. In these "triple difference" (DDD) models, the estimator is $((EY_{T1} - EY_{T0}) - (EY_{C1} - EY_{C0}))/\Delta t_{0,1}$ -

[7] In a recent unpublished working paper, Abadie and Imbens (2009) derive the asymptotic standard error for the PSM estimator of the average treatment effect (considering nearest M neighbor matching with replacement), taking into account the fact that the propensity scores are estimated. Remarkably, they find that the standard error is less when the propensity score is estimated, indicating that use of uncorrected standard errors will lead to conservative inferences when rejecting the null hypothesis (i.e., the true probability of falsely rejecting the null hypothesis will be less than the p-value of the test). However, they note that this result is only for the population average treatment effect and need not apply to the variance of the ATT (*op cit.*, p. 8), which is what we are interested in estimating.

$((EY_{T0} – EY_{T,-1}) – (EY_{C0} – EY_{C,-1}))/\Delta t_{-1,0}$, where EY_{T1}, EY_{T0}, EY_{C1}, and EY_{C0} are as defined earlier; $EY_{T,-1}$ and $EY_{C,-1}$ are the mean outcomes for the treated and control groups during the period -1 (prior to period 0, with both periods 0 and -1 before program implementation); $\Delta t_{0,1}$ is the number of years between period 0 and period 1, and $\Delta t_{-1,0}$ the number of years between period -1 and period 0. The changes in outcomes are divided by the number of years to estimate per year trends, since a different number of years were used to compute the trends in the pre-program period and the pre to during program period. In our analysis, $\Delta t_{0,1} = 5$ and $\Delta t_{-1,0} = 2$ in most cases (except for outcome variables for which we have data only back to 2001, for which $\Delta t_{-1,0} = 1$). This form of DDD estimator has been implemented in a few other studies (McKenzie and Mookherjee 2003; Banerjee, Duflo and Munshi 2003).

One important drawback of all of these estimators of program impact is that they do not account for the level of program investment. These quasi-experimental methods only estimate mean differences in outcomes between program participants and non-participants, as if all participants received the same program funding. Presumably, the impacts of a program are likely to be larger for participants that received more funding. We investigated this issue using switching regression models for matched DRA and non-DRA counties, which also address the bias caused by imperfect matching and allow for heterogeneous impacts of DRA spending depending on the levels of other covariates.[8] The matching counties used in the switching regressions were based on the PSM-NN model without replacement.

The switching regression models have the following form:[9]

(1) $\Delta Y_{Ti} = \alpha_T + \beta_T(P_{Ti} - \mu_p) + \gamma_T(X_{Ti} - \mu_x) + \varepsilon_{Ti}$ for program participants (t), and
(2) $\Delta Y_{Cj} = \alpha_C + \gamma_C\left(X_{Cj} - \mu_x\right) + \varepsilon_{Cj}$ for nonparticipants (c).

[8] The switching regression model was also used to test for the significance of such heterogeneous impacts, using a Chow test for differences in coefficients of the covariates in the regressions for DRA vs. non-DRA counties. This test is the same as the parametric test for heterogeneous program impacts proposed by Crump, et al. (2008). In almost all cases, this test strongly rejected the null hypothesis of homogeneous impacts; so the heterogeneous switching regression model was used.

[9] Note that equations (1) and (2) do not specify that the changes in outcomes (ΔY) are functions of the changes in covariates (ΔX), as in Wooldridge (2002, p. 284), but rather as functions of pre-program values of the covariates (X). The reason for this specification is the endogeneity of ΔX; i.e., changes in values of covariates, such as changes in population and in the economic and demographic structure of the counties studied, could be affected by the DRA program, potentially biasing the estimation results. Furthermore, ΔX is not observed for all relevant covariates, many of which are observed only during decennial census years. Equations (1) and (2) represent a reduced form specification in which the ΔX are derived as linear functions of their pre-program values X and the effects of the program (i.e., ΔX = f(X, P)), and these linear functions substituted into the structural linear model of ΔY (ΔY=g(ΔX,P)=g(f(X,P),P)=h(X,P)). It is not possible to identify the parameters of the structural model g(ΔX,P) (and in particular the structural model impact dg/dp) based on estimation of h(X,P) without restrictive assumptions. Nevertheless, the impact of P on ΔY estimated using h(X,P) (i.e., dh/dP) is of interest in its own right, as the impact of the program conditional on initial conditions (but not conditional on the contemporaneous values of the covariates). Our specification of equations (1) and (2) is similar to the form specified by Abadie (2005, equation (8)).

ΔY_{Ti} is the change in per capita outcome Y from before to during the program for program participant I (i.e., $Y_{T1i} - Y_{T0i}$, using the notation for periods used earlier); ΔY_{Cj} is the change in per capita outcome Y from before to during the program for program nonparticipant j; P_{Ti} is the level of program investment per capita during the program period for program participant i; μ_p is the mean level of program investment per capita in the matched populations of treated and control units; X_{Ti} is a vector of pre-program characteristics of program participant i that influence ΔY_{Ti}; X_{Cj} is a vector of pre-program characteristics of program participant j that influence ΔY_{Cj}; μ_x is the mean of X in the matched populations; α_T, α_C, β_T, γ_T, and γ_C are parameters to be estimated; and ε_{Ti} and ε_{Cj} are error terms with $E(\varepsilon_{Ti})=0$ and $E(\varepsilon_{Cj})=0$. Although linear functional form restrictions are imposed in this model (unlike the simple DD-matching estimator model, which imposes no restrictions on the relationship between ΔY and X), these regression functions allow for heterogeneous impacts of the covariates X on outcomes (i.e., γ_T and γ_C are not necessarily equal). Subtracting the means of P and X ensures that the difference between the intercept terms in regressions (1) and (2) ($\alpha_T - \alpha_C$) estimates the average treatment effect of the program (Wooldridge (2002), p. 613).[10] In estimating these regressions, the population means of P and X are replaced by the sample means.[11]

Two versions of the regressions were run. In Regression 1, the coefficient of the program level variable (β_T) was restricted to equal zero, to mimic the approach of the matching estimators by accounting only for whether units are program participants, but not the level of program funding. In Regression 2, β_T is not restricted, but estimated.

Study population and units of observation

The population and units of observation for this study include nonmetropolitan DRA recipient counties and other nonmetropolitan counties in the eight DRA states and in three additional states of the southeastern United States – Georgia, South Carolina and North Carolina. These states were used to identify matched counties to compare to DRA recipient counties because of their similarities to the Delta region (especially the southern Delta region) in terms of outcome indicators such as income per capita and poverty, in their economic structure, and in their broad historical context. Within the DRA region, DRA-eligible counties that did not receive DRA program funding during the time period studied (2002 to 2007) were not included as possible comparison counties because of concerns about spillover impacts of DRA projects on nearby

[10] The population average treatment effect (ATE) is not the same as what is estimated by the matching - DD models, which were used to estimate the average effect of the treatment on the treated (ATT). However, since the switching regression models were run for matched samples, the ATE and ATT are likely to be similar. Estimation of the ATT using switching regression models requires additional calculations (Wooldridge 2002).

[11] Formally, the use of sample means rather than population means affects the standard errors of the estimates, although this typically has a minor effect on the estimated standard errors (Wooldridge 2002, p. 613). We do not correct our standard errors for this additional source of error.

DRA-eligible but non-recipient counties, which could make such counties a poor choice to represent the counterfactual non-program situation. Concerns about selection bias are also greater in comparing DRA recipient to eligible non-recipient counties, since eligible non-recipient counties may be different from recipients in important but unobserved ways, such as in their ability to organize to obtain and manage project funding.

In total, there are 196 non-metro counties among the 252 DRA-eligible counties. Of these, 133 received DRA funds during 2002 to 2007 through various projects. Two of these counties are also part of the Appalachian Regional Commission (ARC). To avoid confounding impacts of the DRA with impacts of the ARC, we excluded these two counties from the analysis, as well as 131 non-DRA nonmetropolitan ARC counties from the pool of potential control counties. The resulting population included 131 DRA recipient counties and 330 non-DRA eligible counties in the 11 states included in the study. The common support requirement used in the matching eliminated 28 of the DRA recipient counties, leaving 103 DRA-recipient counties in the study sample. The DRA recipient counties that failed to meet the common support requirement were counties having significant rice harvested area. The per capita areas harvested of cotton and rice were included because of strong trends in commodity prices during the study period that could have affected changes in relative farm earnings in DRA vs. non-DRA counties, especially for rice.[12] Rice production was very limited in the study counties outside of the DRA region, so it was not possible to find good matches for major rice producing counties. Hence, our findings cannot be interpreted as applying to all DRA counties, but rather are limited to DRA counties without significant rice area.

Variables and specifications

The outcome variables (Y) investigated include county level personal income per capita and its components (net earnings, current returns to assets (dividends, interest and rent), and personal transfer payments); employment per capita; and population. Within earnings, impacts on total wages and salaries per capita were investigated. We also investigated impacts on earnings and employment per capita by major industry classification for the seven largest industries in nonmetropolitan counties of the Delta Region (construction, manufacturing, retail, education, health care and social services, farming and government).[13] More than 70 percent of the adult population in the nonmetropolitan DRA recipient counties was employed in these industries in

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[12] In initial analysis of the data, these variables were not included among the covariates and substantial differences in growth of farm earnings between the treatment and control counties were found. Inclusion of these covariates in the matching procedures substantially reduced these differences.

[13] Due to missing values of earnings and employment in education for many counties, however, we were not able to conduct the analysis for these outcome variables. There are also many missing observations of earnings and employment for other industries, including construction, manufacturing, and health care and social services. We report the number of observations used for these variables in the results.

2000, with more than 5 percent of adults employed in each (Annex Table A2). We also investigated impacts on different types of transfer payments (retirement and disability, medical, income maintenance, unemployment insurance, veterans' benefits, and federal education and training assistance).

The covariates (X) included in the analysis include many of the same variables used in other studies of impacts of rural interventions on rural economic growth (e.g., Isserman and Rephann (1995), Stenberg, et al. (2009)), including indicators of prior outcomes (personal income per capita in 2000, the poverty rate in 2000, shares of personal income from asset returns and from transfer payments in 2001, population in 2000), economic structure (share of adults in 2000 employed in the seven largest industries)[14], and spatial structure (distances to the nearest urban center of different sizes in 1980 (25,000 or more; 100,000 or more; 250,000 or more; 500,000 or more; 1 million or more) and population density in 1990).

Additional covariates were included in the analysis because these were also judged to possibly differ between DRA recipient counties and non-DRA counties, and to be potentially important determinants of changes in outcomes. These covariates included indicators of the demographic and educational structure of the population in 2000 (rural share, farm household share, African American share, share age 17 or less and share age 65 or more, share of adults with more than a high school diploma), employment conditions in 1999 (share of men and share of women working full time all year), cotton and rice areas harvested per capita in 2002, Federal economic development grant funds received per capita during 2000-01, and whether the county was in a Gulf Opportunity Zone. Federal economic development funding is a potentially important confounding factor, since such funds may augment or displace funds provided by the DRA. Failure to account for this (and other) confounding factors could have biased the conclusions of prior studies of the impacts of particular economic development interventions. The dummy variable for Gulf Opportunity Zone counties was included to account for potential impacts of Hurricanes Katrina and Rita, and the effects of the Katrina Emergency Tax Relief Act of 2005 (KETRA) and the Gulf Opportunity Zone Act of 2005 (GO Zone), which provided tax incentives to stimulate recovery and development in the regions affected by these hurricanes.[15]

The analysis was conducted using both untransformed linear variables and a version with transformed variables, using changes in logarithms of the dependent variables ($\Delta \log Y$) and logarithms of continuous and positive covariates. The results were qualitatively similar using

[14] Unlike Isserman and Rephann (1995) and Stenberg, et al. (2009), we use shares of employment in different industries rather than shares of income, because of missing (undisclosed) values of earnings by industry in the Bureau of Economic Analysis data for many nonmetropolitan counties.

[15] Of the twelve counties most affected by flooding resulting from Hurricane Katrina, only one – Tangipahoa Parish in Louisiana – is a nonmetropolitan county. Excluding this parish from the analysis had little impact on the results.

both approaches. To save space and to facilitate interpretation of the results, we report only the results using the untransformed variables.[16]

Data

The data on personal income and employment and their components and on population by county were taken from the Regional Economic Information System (REIS) of the Department of Commerce Bureau of Economic Analysis (BEA) (http://www.bea.gov/regional/reis/). The estimates of personal income and employment are based on administrative records, censuses, and surveys, and are designed to be consistent with state and national levels of personal income reported the National Income and Product Accounts.[17] For total personal income and employment and major components of personal income and employment, the data are available by county from 1969 to 2007. For earnings and employment by industry, the data are only available from 2001 to 2007. This affects the baseline year used in the analysis of pre-2002 trends and for the DDD estimator, with a different baseline year used for earnings and employment by industry.

The data on poverty rate and demographic and education characteristics of counties in 2000 and employment conditions in 1999 were taken from the 2000 Census of Population (http://www.census.gov/main/www/cen2000.html). The data on areas of cotton and rice harvested in 2002 were taken from the 2002 Census of Agriculture (http://www.agcensus.usda.gov/Publications/2002/).

The data on distances to urban centers of different sizes and population density were provided by Peter Stenberg, and were based on geographic information systems analysis conducted by researchers of the Economic Research Service as part of a study of broadband internet in rural areas (Stenberg, et al. 2009).

The data on economic development grant spending in 2000 and 2001 was taken from the Consolidated Federal Funds Report (CFFR) of the Census Bureau (http://www.census.gov/govs/cffr/). Classification of specific federal programs as rural economic development programs used the classification developed by the U.S. Government Accountability Office (GAO) in a recent report on federal rural economic development programs (GAO 2006). This GAO report notes several problems with the data that are reported in the CFFR, but this is the only comprehensive source available for these programs.

The data on DRA spending by county were taken from the DRA's Federal Grant Program Profile (DRA 2009), which lists all DRA projects funded from 2002 to 2008 by year, project name,

[16] The results using transformed variables are available from the authors upon request.
[17] See http://www.bea.gov/regional/pdf/lapi2007/lapi2007.pdf for details on the methodology used to produce the local area personal income and employment estimates.

location and approved funding amount. Since the approved amounts of funding may not be spent in the same year that approval occurred, the amount of funds actually spent in each county during 2002 to 2007 may be less than amounts approved during this period. Despite this concern, these data were judged to be more reliable than the amounts reported as DRA outlays in the CFFR.

The list of GO Zone counties is taken from the Gulf Opportunity Zone Act of 2005.

4. Results

Matching model results

The probit model used to estimate propensity scores for DRA participation is shown in Annex Table A1, and comparisons between the covariates in the unmatched and matched samples are shown in Annex Tables A2 and A3. Not surprisingly, the mean values of many of the covariates differ between DRA counties and non-DRA counties in the unmatched samples. In general, DRA recipient counties were poorer and more dependent upon Federal spending than non-DRA counties in the Delta and southeast states, with a smaller share of the adult population employed and greater dependence on service occupations. Such initial differences may affect differences in outcomes during the study period, and therefore need to be controlled for using econometric methods.

Table A2 indicates that most of these mean differences in characteristics are much smaller in the matched samples using the propensity score – nearest neighbor matching method (PSM-NN) with replacement. Statistically significant differences remain in the matched samples for only a few variables: the share of adults employed in manufacturing (less in DRA counties), cotton harvested area per capita (more in DRA counties) and the elderly share of the population (less in DRA counties). In all of these cases, the statistical significance is weak (between 5% and 10% level) and the mean differences are relatively small. Across all covariates, the maximum absolute standardized bias is reduced from over 100% to 27%.[18] The pseudo R^2 of the probit model is much lower in the matched sample, and a likelihood ratio (LR) test of overall balance in the matched sample indicates that differences in the covariates are statistically insignificant, with a p value of 0.103.[19] Hence, this matching method performs well

[18] The sample standardized bias for covariate X is defined as $(m(X_t)-m(X_c))/\text{square root}(s_t^2+ s_{cr}^2)$, where $m(X_t)$ and $m(X_c)$ are the sample means for the treated and control groups (whether matched or unmatched), respectively; and s_t^2 and s_{cr}^2 are the sample variances for the treated group and control reservoir (unmatched controls), respectively (Rosenbaum and Rubin 1985). The standardized bias is divided by this denominator (rather than the variance of the difference in means, as in a t statistic) so that the measure is not affected by sample size and is comparable between different matching methods.

[19] The overall balance test is a likelihood ratio test of the joint statistical significance of all covariates in a probit model for program participation in the matched sample. If the samples are well matched, the covariates should have a statistically insignificant impact in this model.

to reduce if not eliminate all differences between the DRA recipient counties and the matched non-DRA counties in their pre-DRA characteristics.

Table A3 provides similar comparisons between the matched samples using the other matching methods investigated.[20] This matching estimator results in larger biases for some variables (with a maximum absolute bias of nearly 36%) and more statistically significant differences (compared to matching with replacement), because the constraint of non-replacement limits the ability to use the best matching counties more than once. With this estimator, there are statistically significant differences between the DRA and matching non-DRA counties in terms of the poverty rate (greater in DRA counties), the share of adults employed in manufacturing (less), whether the county is in a GO Zone (more likely), rice harvested area per capita (greater), the farm share of the population (less), the child share of the population (greater), and the share of women working full time all year (less). Despite having larger biases and more significant differences for several individual covariates, the PSM-NN estimator without replacement has a lower overall measure of bias, with a smaller pseudo R^2 and smaller LR test statistic than the PSM-NN estimator with replacement. Hence it is not clear whether the PSM model with or without replacement is preferable.

The PSM kernel matching (PSM-KM) estimator performs the best, with no statistically significant mean differences for any covariates, the smallest maximum bias (24%), the smallest pseudo R^2 and the smallest LR test statistic. The Mahalanobis metric (MM) estimator performs the poorest in terms of bias, with significant differences remaining between the DRA and matched samples for twelve of the covariates, the largest maximum bias (nearly 62%), and the largest pseudo R^2 and LR test statistic (indicating statistically significant difference overall between the matched samples).

These results are consistent with results of other studies that compare different matching methods (Gu and Rosenbaum 1993; Zhao 2004), and demonstrate that no matching method is clearly superior in terms of both bias reduction and efficiency. Hence, as noted earlier, we report the results of several methods and investigate the robustness of our conclusions to the method.

DD estimates with matching

The results of the estimation using the DD estimator for changes in the outcome measures using the different matching methods are reported in Table 2. We find that growth in per

[20] The comparisons between unmatched samples do not vary across the matching methods, so these comparisons are not shown again in Table A3. The mean levels of all covariates for the DRA counties are the same for all matching methods, so these are reported only once in Table A3 for comparison purposes. The difference between these matching methods is in their choice of matched non-DRA counties.

capita personal income from 2002 to 2007 was greater in the DRA counties than in the matched non-DRA counties, with the difference statistically significant (at the 10% level or less) for four of the six matching estimators. In all cases the mean difference was in the range of $500 to $660 per capita, a fairly large difference. This difference was not statistically significant using either bias corrected estimator, however. This is due mainly to larger standard errors of the bias corrected estimators.

Changes in personal income per capita for the DRA recipient counties (with common support) and the matched non-DRA non-metro counties are shown in Maps 2 and 3 (using PSM-NN without replacement). No strong geographical pattern of changes in personal income is evident for either group. Comparing the cumulative distribution of changes in per capita personal income for DRA-recipient and non-DRA counties indicates that the distribution of income per capita of DRA-recipient counties stochastically dominates that of matched non-DRA counties (Figure 2). Thus, it is evident that the mean difference in income growth per capita is not driven by outliers in these distributions. Furthermore, comparison of these distributions suggests that the impact of the DRA is stronger at lower levels of income per capita, indicating a tendency towards more pro-poor impacts.

Among the major components of personal income (net earnings; dividends, interest and rent; and transfer payments), transfer payments grew significantly more rapidly in the DRA counties, using four of the six estimators. The difference in growth in transfer payments according to the PSM-NN estimator with replacement was not significant, in part because the standard errors tend to be larger for this estimator, as discussed earlier. For all major income components the predicted sign of the difference was positive (i.e., greater growth in DRA counties), although the differences were not statistically significant except for transfer payments.

Looking at changes in earnings per capita by industry, we find no statistically significant differences that are robust across the estimators (the growth in farm earnings per capita was greater in DRA countries and statistically significant at the 10% level using the PSM-NN estimator without replacement, but not significant using other estimators). Similarly, differences in growth in employment per capita in total and in most major industries were not statistically significant and robust across estimators. Employment per capita in farming grew more rapidly in DRA counties according to a few estimators (PSM-NN without replacement and MM-NN, with and without bias correction), but this difference was not significant using other estimators.

Among the different types of transfer payments, the difference between DRA counties and matched non-DRA counties was largest and most robust for medical transfer payments. The estimated mean differences in growth in medical transfer payments were positive and statistically significant for all estimators except PSM-NN with replacement. Considering different types of medical transfer payments, mean growth in Medicare payments was greater in DRA counties, with the difference statistically significant using four of the six matching estimators. Mean growth in Medicaid, SCHIP and other state medical transfer payments was also greater in DRA counties in all cases, but the difference was statistically significant only for one of the estimators (MM, bias corrected).

Growth in income maintenance program payments was greater in DRA counties according to most of the estimators. The largest and most robust differences were for growth in food stamp payments. Still, these differences were much smaller than the differences in medical transfer payments.

Population growth was less (or population decline was greater) in DRA counties according to some estimators (PSM-NN without replacement, MM-NN with and without bias correction). There was no statistically significant difference between DRA counties and matched non-DRA counties in the change in the share of the population that is elderly, according to any of the estimators. Hence, the changes in population growth or difference in growth of Medicare transfer payments in DRA counties does not appear to be driven by differences in growth of the elderly population. We find more growth in the share of the population that is African American in DRA counties using two of the estimators (PSM-NN without replacement in MM-NN without bias correction). This could be related to the greater decline in population observed in DRA counties using those same estimators (i.e., greater decline in the white population), and could be related to differences in growth in food stamps per capita, to the extent that African Americans are poorer and more likely to use food stamps in the region studied. These are not necessarily effects of the DRA, however, although these tendencies are more apparent in DRA-recipient counties.

To try to better understand the differences in medical transfer payments, we also investigated changes in the supply of medical staff and facilities, considering the number of doctors, nurses and hospital beds per capita (also reported in Table 2). We find no statistically significant differences between DRA counties and matched non-DRA counties in the change in number of doctors or nurses per capita using any of the estimators, and insignificant differences in the change in number of hospital beds per capita using all but one estimator (PSM-NN without replacement). These results do not explain the finding of more growth in medical transfer payments in DRA-recipient counties.

Pre-DRA differences in outcome trends

Table 3 provides estimates of the differences between DRA recipient counties and matching non-DRA counties in their pre-2002 outcome trends. For most outcome variables and most matching estimators, there were not statistically significant differences in these pre-2002 outcome trends. Here we comment on outcome variables for which there was a significant difference using at least one of the matching estimators.

Pre-2002 growth in per capita personal income, net earnings, transfer payments, earnings from farming, total employment, government employment, and medical transfer payments was more rapid in the DRA recipient counties than matched non-DRA counties according to the uncorrected MM estimator. However, none of these differences was significant using any of the other estimators. Given the large biases noted earlier for the MM estimator and the lack of robustness of these results, these results are not substantial evidence of a difference in these trends prior to 2002.

Considering earnings per capita by industry, we find that growth in manufacturing earnings per capita from 2001 to 2002 was greater in DRA recipient counties using two of the matching estimators (PSM-NN without replacement and PSM-KM with replacement). Growth in earnings in the health care and social services industry was greater (with weak statistical significance) according to one estimator (MM – bias corrected). Growth in government sector earnings was greater according to two estimators (MM, both uncorrected and bias corrected).

Considering employment by industry, the pre-2002 growth in manufacturing employment was more rapid in DRA recipient counties according to two estimators (PSM-NN without replacement and MM – uncorrected), but less rapid according to the MM – bias corrected estimator. Growth in employment in health care and social services was more rapid (weakly significant) according to the MM – bias corrected estimator. Farm employment growth was less rapid according to the MM – bias corrected estimator.

Growth in income assistance program payments in total and in family assistance program payments was significantly greater in DRA recipient counties using all matching estimators. Growth in Supplemental Security Income (SSI) payments and in Food Stamps was significantly greater according to two of the estimators (PSM-NN without replacement and MM – uncorrected). Unemployment insurance payments grew less rapidly in DRA recipient counties (weakly significant) according to the same two estimators.

Population grew less rapidly (or declined more rapidly) in DRA recipient counties from 2000 to 2002 according to two of the estimators (PSM-NN without replacement and MM – uncorrected).

These results indicate that there may have been differences between DRA recipient counties and matched non-DRA counties in outcome trends prior to implementation of the DRA for some outcome variables. However, few of these differences are robust to the choice of estimator, with many of these seen only with the uncorrected MM estimator or the PSM-NN estimator without replacement. Nevertheless, these results raise concerns about possible biases in the DD estimates reported in Table 2 due to differences in prior outcome trends, especially for changes in family assistance program payments. To address this concern, we used the DDD estimator to net out prior trends from the DD estimates.

DDD estimates with matching

The results of the triple difference estimation are reported in Table 4. The results show few statistically significant impacts of the DRA on changes in outcome trends. Unlike in Table 2, we do not find a significant impact of the DRA on growth in personal income per capita overall, and the impact on total transfer payments per capita is significant only using the MM-NN estimator with bias correction.

As in Table 2, we find insignificant impacts on earnings and employment by industry in most cases. In some industries (manufacturing, farming, and government), we find less growth in earnings using at least one estimator, although these results are not robust across most estimators. The most robust finding for changes in earnings by industry is that earnings in government activities declined more in DRA recipient counties, with this difference statistically significant according to three of the estimators (PSM-NN with replacement and bias correction, PSM-KM and MM – uncorrected).

We find that employment growth was less in DRA counties in manufacturing according to two of the estimators (PSM-NN without replacement and MM – uncorrected), and less in government according to the MM – uncorrected estimator. By contrast, we find that growth in farm employment is greater in DRA recipient counties according to two estimators (both bias corrected PSM and MM).

Consistent with Table 2, we find statistically significant and robust impacts of the DRA on Medicare payments. Unlike Table 2, however, we find a significant and highly robust negative impact of the DRA on trends in family assistance payments, with greater decline in such payments in DRA counties compared to matched non-DRA counties, after subtracting prior trends. Growth in unemployment insurance payments was greater (weakly significant) in DRA recipient counties according to two of the matching estimators (PSM-NN without replacement and MM – uncorrected), and growth in veterans benefits was greater according to the PSM-KM estimator only.

The association of DRA counties with population decline observed using some matching estimators in Table 2 is not found using the DDD estimator. Indeed, DRA counties are associated with more increase in population growth using some of the matching estimators with the DDD estimation. Hence, differences in prior population growth trends account for the apparent negative impact of the DRA on population growth reported in Table 2, rather than an actual negative impact.

These results suggest that differences in prior trends (and the unmeasured factors responsible for them) do not account for differences in trends during the DRA implementation period for most outcome variables. Exceptions include family assistance program payments, which were growing more rapidly in DRA counties prior to DRA implementation but did not do so afterward; and population, which was growing more slowly in DRA counties prior to DRA implementation according to some estimators, but which grew more rapidly during DRA implementation after subtracting the prior trend. For these exceptions, the DRA may have caused a change in prior trends, reducing the prior growth in family assistance payments but increasing population growth. For other variables, there is less evidence that any differences observed during the DRA implementation period were due to prior trends. In particular, the robust finding that medical transfer payments grew more rapidly in DRA counties between 2002 and 2007 cannot be explained as a continuation of a prior difference in trends.

Switching regression estimates with matched sample

The results of Regression 1 are similar to those of the matching estimators reported in Table 2 (Table 5). Personal income per capita grew more rapidly in DRA counties (by about $518 per capita on average), as did personal transfer payments per capita (both strongly significant) and net earnings per capita (weakly significant). There are few significant differences in earnings per capita by industry (except a weakly significant result for farm earnings, which grew faster in DRA recipient counties, and government earnings, which grew more slowly in DRA counties). There are no significant differences in total employment per capita or employment by industry. As with the matching analysis, we find significant differences in transfer payments per capita, especially medical transfer payments, but also including retirement and disability payments, Food Stamps and veterans benefits. Also consistent with the matching analysis, we find evidence of slower population growth (or more population decline) in DRA counties using Regression 1.

In Regression 2, we find that several outcomes are associated with greater DRA spending, controlling for whether or not the county is a DRA county. Personal income per capita grew significantly more in counties with more DRA spending per capita, with each $1 of additional DRA spending per capita associated with $15 of additional growth in personal income per capita. This suggests that DRA spending is having a strong impact on personal income growth, well beyond the simple amount of funds transferred, indicating that other funds are being leveraged and/or that economic growth is being stimulated. The results for net earnings (almost $8 of additional net earnings growth per $1 of DRA spending per capita) and wages and salaries (almost $12 per capita of additional wages and salaries for each additional $1 of DRA spending per capita) suggest that DRA spending is having a strong marginal impact on economic activity, and is not only having an effect by leveraging transfer payments.

Across major industries, we find a significant impact of additional DRA spending only for the health care and social services sector, with each $1 of additional spending associated with more than $8 of additional earnings.[21] These impacts were apparently masked in the matching analysis, which failed to account for differences in the level of DRA spending.

Although the impacts of the DRA apparently go beyond transfer payments, DRA spending is also having a strong impact on transfer payments, with each additional $1 of DRA spending associated with $5 of additional transfer payments. The additional transfer payments are mainly medical transfers ($2.49 per $1 of additional DRA spending) and retirement and disability benefits ($1.67 per $1 of additional DRA spending). These findings suggest that DRA spending leverages other forms of government spending. In the case of medical transfer payments, this is likely due to DRA investments in medical facilities, which is one of the major components of the DRA projects that have been funded. This is consistent with the fact that the health care sector is the only one found to have greater earnings as a result of greater DRA spending, and suggests that a major near term impact of the DRA has been to promote health sector earnings and transfer payments through investments in medical facilities. This is also supported by the fact that the number of hospital beds per capita has grown more in counties where DRA spending was greater (also reported in Table 5).

Controlling for the level of DRA spending per capita, as in Regression 2, the coefficient of the DRA county dummy reflects differences in outcome trends between DRA counties and matched non-DRA counties that are not due to DRA funding levels. Such differences could be due to the fact that the DRA is operating in DRA counties, regardless of funding levels (e.g., as a result of improved organizational capacity in DRA counties), though it may also reflect unmeasured differences between DRA-recipient and matched non-DRA counties.

Robustness of results to different baseline and ending years

We investigated the robustness of the matching DD estimation and regression results to different baseline and ending years, considering 2001 as an alternative base year and 2005 and 2006 as alternative ending years for the estimations.[22] We chose 2002 as the baseline (pre-project implementation) year in the analysis because DRA outlays were minimal in 2002, as shown in Table 1. Nevertheless, we consider 2001 as an alternative baseline year, given that some program impacts may have begun to occur in 2002.

[21] The results for the health and social services sector are based on a constrained regression (with equal coefficients of the covariates for both DRA and non-DRA counties), because of a small sample size due to data suppression for earnings in this sector. Given the small sample size, we have less confidence in the results for this sector than for others.

[22] These results are not reported to save space, but are available upon request from the authors.

The results were qualitatively very similar and in some cases more robust across estimators when using 2001 as the base year or 2006 as the ending year. All of the matching and regression estimators with DD showed statistically significant greater growth in personal income per capita in DRA recipient counties when 2001 was taken as a base year, and most estimators showed greater growth from 2002 to 2006, with all of the estimated impacts of the same order of magnitude as those shown in Tables 2 and 5. Differences in earnings growth by industry were again statistically insignificant in almost all cases, except for farm earnings, which grew more from 2001 to 2007 and from 2002 to 2006 in DRA recipient counties according to most of the estimators. The growth in transfer payments during these periods was greater in DRA counties according to almost all estimators and was greater where DRA spending was greater, as in Tables 2 and 5. The difference in growth in transfer payments in DRA counties from 2001 to 2007 and from 2002 to 2006 is mostly due to greater growth in medical transfer payments, as in Tables 2 and 5, although there are differences in growth of other transfer payments as well, particularly in food stamps. Also as in those tables, there are few statistically significant differences across estimators in changes in returns to assets, or in employment per capita in total or by industry. And the estimated population change was smaller in DRA counties according to some of the estimators (the same ones showing significant negative coefficients in Tables 2 and 5).

Considering 2005 rather than 2006 or 2007 as the ending year, the differences in growth of total personal income per capita were no longer statistically significant, although transfer payments grew more rapidly in DRA counties during this period according to most of the estimators. It appears that earnings and overall income growth responded more slowly to DRA investments than transfer payments. As when using other starting and ending years, differences in growth of medical transfer payments was the major source of difference between DRA and matching non-DRA counties. Differences in other outcomes were also qualitatively similar to the differences observed for 2006 or 2007 as the ending year.

These results give us confidence that the results reported in Tables 2 and 5 are not statistical artifacts evident only in one particular time period, but are robust to alternative specifications of the starting and ending year used for the comparisons. They also suggest that the impacts of DRA investments on broader measures of economic growth, such as on earnings and total personal income per capita, take longer to become evident than the impacts on government transfer payments.

Summary

Overall, the matching and regression results suggest that the DRA is having a positive impact on growth in personal income per capita in DRA counties by increasing transfer payments, especially medical transfer payments. Total earnings and employment per capita did not increase measurably more on average in DRA counties, but among DRA counties, earnings increased more in counties where DRA spending per capita was greater. The evidence suggests that earnings increased mainly in the health care and social service sector in counties with higher DRA spending per capita. The DRA has had unclear impacts on changes in population. Although some estimators suggest that population declined more in DRA counties, the evidence showed that this trend existed before the DRA was implemented, and a positive impact on population growth is indicated by those same estimators when the prior population trends are subtracted out. The evidence also suggests that it takes longer for some impacts than others to be observed; e.g., the impacts on transfer payments are evident earlier than impacts on earnings and total personal income per capita.

5. Conclusions

The results of this analysis suggest that even though the DRA is a relatively small program and its impacts could only be investigated during its first five years of implementation, the program has had measurable positive impacts on some outcomes, including per capita personal income and transfer payments. These impacts were larger in counties where DRA spending per capita was greater, with each $1 of additional DRA spending associated with an additional $15 in personal income per capita, including $8 in additional earnings in the health care and social service sector and $5 in additional transfer payments – mainly due to additional medical transfer payments. The effect of higher DRA spending on health sector earnings and medical transfer payments is consistent with the fact that spending on medical facilities is one of the highest priority areas of DRA spending, and with the fact that the number of hospital beds per capita has grown more rapidly in counties with more DRA spending per capita.

The fact that we do not find measurable impacts of the DRA so far on other outcome indicators, such as on earnings per capita in most industries or on employment per capita, does not mean that there have not been any such impacts or will not be in the future. Given the relatively small amount spent by the DRA in DRA recipient counties so far and the time required for investments in infrastructure to affect economic growth, it is not surprising that it is difficult to detect impacts after only five years of program implementation. Furthermore, some of the largest investments made by the DRA have been in community facilities such as improved water and sewer systems, which improve the quality of life but may have little direct near-term impact on employment or income, although they may promote community economic development in the longer term by attracting new residents and industries and reducing outmigration.

It is perhaps more surprising to find such large incremental impacts of DRA spending on personal income, earnings and transfer payments. The results suggest that these impacts are not simply the direct result of DRA funds circulating in the local economies of the Delta Region, since the multipliers are far larger than those typically estimated for spending in rural areas. Rather, it appears that DRA spending on medical facilities is leveraging additional resources through medical programs such as Medicare and Medicaid, and that these are contributing to increased earnings in the health care and social service sector.

The estimated impacts of the DRA on income in the health care sector raise the general question of the potential for investments in that sector to contribute to sustainable economic development in rural areas. Ours is not the first study to notice the potential economic impacts of health sector investments in rural areas of the United States. There is a growing body of literature on such impacts, led by researchers at National Center for Rural Health Works at Oklahoma State University and other research centers (e.g., Doeksen, et al. 1998; Doeksen and Schott 2003; St. Clair, Doeksen and Schott 2007; St. Clair and Doeksen 2009). However, most of that literature estimates impacts using an economic input-output model, without being validated by empirical *ex-post* estimates of the impacts of actual investments. The present study is the only one that we are aware of that estimates such impacts using quasi-experimental and other econometric methods with county level income data. Although we did not start out specifically hypothesizing impacts of the DRA on income in the health sector, the fact that we found such impacts suggests that further research on the impacts of this type of investment in rural areas, using methods such as those used here, could prove fruitful.

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Table 1. Appropriations and Outlays for Selected Regional Development Programs ($ million)

Fiscal Year	Appropriations[23]	Outlays[24]			
	DRA	DRA	EDA	ARC[25]	Denali Commission
1999	-	-	355	136	1
2000	-	-	356	125	38
2001	20.0	-	356	86	11
2002	10.0	1	355	101	-14[26]
2003	7.9	6	375	74	2
2004	5.0	12	337	68	16
2005	6.0	9	332	65	49
2006	11.9	6	284	63	42
2007	11.9	8	243	67	33
2008	11.7	8	238	69	46
2009	13.0	9	243	62	60
2010[27]	13.0	13	422	65	79

[23] Source: Annual appropriations bills, various years.
[24] Source: Table 12.3, Historical Tables from the President's Budget, FY2011. Available at: (www.whitehouse.gov/omb/budget/Historicals/).
[25] Excludes Appalachian Highway Program.
[26] Negative number due to de-obligated funds.
[27] Figures for 2010 are estimates.

Table 2. Mean changes in outcomes, 2002 to 2007, DRA minus matching counties (DD estimator) (standard errors in parentheses)

Dependent Variable	**PSM-NN With replacement**		**PSM-NN Without replacement**	**PSM-KM With replacement**	**MM-NN With replacement**	
	Not bias corrected	**Bias corrected**	**Not bias corrected**	**Not bias corrected**	**Not bias corrected**	**Bias corrected**
Personal income per capita	604.7* (331.2)	539.8 (386.4)	660.3*** (248.4)	597.0* (333.5)	498.9* (281.5)	619.0 (448.7)
Major components of personal income						
- Net earnings per capita	278.5 (283.1)	225.5 (316.4)	324.7 (249.9)	240.2 (229.1)	192.9 (210.6)	175.9 (329.0)
- Wages and salaries per capita	160.4 (395.7)	12.7 (274.9)	157.7 (195.6)	212.1 (293.9)	177.2 (223.6)	148.6 (277.6)
- Dividends, interest and rent per capita	170.8 (176.9)	213.0 (142.0)	166.0 (108.8)	164.6 (127.9)	76.6 (130.6)	82.5 (173.9)
- Personal transfer payments per capita	155.3 (120.7)	101.0 (85.4)	169.5** (71.1)	192.1** (81.5)	229.4*** (68.3)	360.5*** (93.7)
Earnings per capita by industry						
- Construction	9.7 (123.9)	-66.6 (160.7)	-62.3 (92.0)	-69.5 (112.7)	-18.2 (98.5)	80.4 (105.7)
- Manufacturing	152.9 (221.5)	228.9 (265.5)	-29.4 (157.6)	179.3 (184.6)	-82.4 (192.9)	-250.4 (281.1)
- Retail	-18.0 (35.2)	3.5 (28.8)	1.8 (31.5)	-3.9 (25.9)	23.0 (27.5)	36.8 (40.3)
- Health care and social services	83.8 (123.8)	-162.3 (171.9)	88.1 (76.5)	106.3 (143.3)	120.6 (91.6)	-58.7 (146.9)
- Farming	112.7 (97.2)	-17.8 (131.9)	144.9* (76.4)	124.2 (92.3)	32.9 (103.3)	134.5 (116.9)
- Government	-28.2 (220.3)	-123.3 (161.7)	-49.4 (105.7)	-87.6 (148.5)	63.6 (55.5)	46.5 (60.2)
Employment per capita	-0.0022 (0.0072)	-0.0060 (0.0064)	-0.0024 (0.0052)	-0.0028 (0.0053)	0.0020 (0.0055)	-0.0039 (0.0075)

Dependent Variable	PSM-NN With replacement		PSM-NN Without replacement	PSM-KM With replacement	MM-NN With replacement	
	Not bias corrected	Bias corrected	Not bias corrected	Not bias corrected	Not bias corrected	Bias corrected
Employment per capita by industry						
- Construction	-0.0013 (0.0020)	-0.0008 (0.0030)	-0.0011 (0.0017)	-0.0018 (0.0020)	0.00004 (0.00187)	0.0022 (0.0022)
- Manufacturing	-0.0009 (0.0044)	-0.0010 (0.0042)	-0.0024 (0.0038)	0.0004 (0.0033)	-0.0011 (0.0041)	-0.0104* (0.0061)
- Retail	-0.0016 (0.0013)	-0.0010 (0.0010)	-0.0007 (0.0009)	-0.0010 (0.0013)	0.0001 (0.0010)	0.0004 (0.0014)
- Health care and social services	-0.0004 (0.0030)	0.0043 (0.0110)	-0.0010 (0.0019)	-0.00001 (0.0017)	0.0003 (0.0017)	-0.0061** (0.0028)
- Farming	0.00032 (0.00055)	0.00055 (0.00042)	0.00064** (0.00031)	0.00029 (0.00052)	0.00077** (0.00036)	0.00088* (0.00051)
- Government	-0.0010 (0.0021)	-0.0010 (0.0015)	-0.0010 (0.0010)	-0.0010 (0.0017)	-0.0008 (0.0014)	-0.0010 (0.0015)
Transfer payments by type						
- Retirement and disability	9.3 (33.4)	25.7 (26.3)	20.0 (24.8)	16.4 (26.8)	38.9* (22.4)	78.4** (32.2)
- Medical	93.0 (77.4)	69.6 (61.9)	111.3** (45.6)	120.5** (59.2)	116.5** (52.3)	258.5*** (71.3)
- Medicare	40.5 (31.4)	59.4** (25.8)	56.2*** (18.3)	49.9** (23.0)	75.8*** (24.2)	43.7 (28.2)
- Medicaid/SCHIP/other state programs	51.3 (70.7)	10.3 (63.0)	56.3 (54.4)	71.0 (62.9)	42.2 (44.5)	213.7*** (71.2)
- Income maintenance	37.6* (20.6)	23.7 (18.1)	25.8* (13.6)	35.3** (16.0)	47.0*** (16.2)	18.2 (13.0)
- Supplemental security income	6.8 (9.7)	7.2 (7.0)	5.2 (4.5)	4.5 (7.2)	8.8** (4.3)	11.8** (4.8)
- Family assistance	-2.7 (3.0)	-4.9 (3.6)	-3.7 (2.6)	-2.3 (2.7)	-3.5 (3.0)	-2.9 (3.0)

Dependent Variable	PSM-NN With replacement		PSM-NN Without replacement	PSM-KM With replacement	MM-NN With replacement	
	Not bias corrected	Bias corrected	Not bias corrected	Not bias corrected	Not bias corrected	Bias corrected
- Food stamps	13.6** (5.6)	11.2* (6.1)	13.2*** (3.9)	13.6*** (5.1)	13.3** (5.3)	7.3 (5.7)
- Unemployment insurance	7.5 (14.6)	-34.2 (24.1)	2.7 (7.2)	6.3 (10.1)	14.2 (8.7)	-9.1 (10.9)
- Veterans benefits	5.3 (10.7)	7.0 (7.7)	6.1 (5.3)	9.6 (6.4)	13.7** (6.9)	12.7 (8.4)
- Federal education and training assistance	5.4 (11.5)	8.5 (17.0)	1.1 (6.0)	2.1 (8.1)	0.9 (6.6)	-1.9 (7.7)
Population	-253.3 (406.1)	-7.6 (431.0)	-449.9** (213.0)	-447.5 (543.0)	-548.2*** (208.9)	-590.4** (243.5)
Share of population over 65 years of age	0.00088 (0.00168)	-0.00082 (0.00160)	0.00041 (0.00099)	0.00084 (0.00157)	0.00072 (0.00138)	0.00173 (0.00176)
African American share of population	0.00153 (0.00240)	-0.00051 (0.00216)	0.00225* (0.00116)	0.00205 (0.00187)	0.00326** (0.00152)	0.00238 (0.00206)
Number of non-federal medical doctors per capita	0.000030 (0.000038)	-0.000039 (0.000038)	0.000025 (0.000027)	0.000024 (0.000023)	0.000034 (0.000034)	0.000030 (0.000041)
Number of registered nurses (FTE) per capita	0.000099 (0.000189)	0.000020 (0.000204)	-0.000150 (0.000130)	-0.000037 (0.000166)	-0.000159 (0.000190)	0.000056 (0.000232)
Number of hospital beds per capita	-0.00036 (0.00042)	-0.00023 (0.00039)	-0.00056** (0.00027)	-0.00055 (0.00034)	-0.00017 (0.00027)	-0.00072 (0.00066)

*, **, *** Difference statistically significant at 10%, 5%, and 1% levels, respectively.

Table 3. Mean changes in outcomes, 2000 or 2001 to 2002, DRA minus matching counties[28]

Dependent Variable	PSM-NN With replacement		PSM-NN Without replacement	PSM-KM With replacement	MM-NN With replacement	
	Not bias corrected	Bias corrected	Not bias corrected	Not bias corrected	Not bias corrected	Bias corrected
Personal income per capita	2.2 (195.9)	219.3 (270.4)	243.7 (152.8)	87.1 (138.1)	506.0*** (169.6)	-53.6 (244.7)
Major components of personal income						
Net earnings per capita	84.5 (171.9)	220.8 (218.9)	182.8 (121.0)	123.8 (141.8)	404.5*** (151.4)	22.6 (204.2)
Dividends, interest and rent per capita	-55.6 (56.4)	-34.0 (53.3)	6.8 (35.4)	-33.2 (33.6)	14.9 (47.2)	-40.1 (65.4)
Personal transfer payments per capita	-26.6 (56.6)	32.6 (50.5)	54.2 (47.8)	-3.3 (41.0)	86.8** (36.2)	-35.9 (43.5)
Major components of earnings						
Wages and salaries per capita	83.3 (125.0)	42.8 (182.2)	133.1 (110.8)	103.0 (111.0)	172.9 (121.1)	-100.7 (151.2)
Earnings per capita by industry						
- Construction	59.6 (90.7)	38.3 (138.5)	29.5 (62.2)	16.3 (87.6)	34.7 (82.3)	-21.4 (82.6)
- Manufacturing	134.7 (116.5)	46.7 (131.0)	139.4** (61.1)	146.6* (88.5)	69.8 (77.3)	-162.2 (160.4)
- Retail	1.1 (15.4)	-14.3 (15.9)	-4.9 (14.0)	-0.5 (14.0)	1.8 (13.6)	-25.0 (21.8)
- Health care and social services	17.5 (24.1)	6.1 (48.4)	4.6 (21.2)	-2.2 (18.4)	20.1 (19.1)	191.9* (115.3)
- Farming	-13.2 (62.9)	116.5 (75.7)	-12.5 (45.6)	-1.6 (51.8)	165.4** (67.1)	41.8 (93.5)
- Government	38.7	46.3	2.1	24.8	49.1***	31.5*

[28] Changes from 2001 to 2002 for earnings and employment per capita by industry; all other changes are from 2000 to 2002.

Dependent Variable	**PSM-NN With replacement**		**PSM-NN Without replacement**	**PSM-KM With replacement**	**MM-NN With replacement**	
	Not bias corrected	**Bias corrected**	**Not bias corrected**	**Not bias corrected**	**Not bias corrected**	**Bias corrected**
	(38.9)	(41.0)	(21.8)	(26.0)	(17.8)	(17.9)
Employment per capita	0.0035 (0.0048)	0.0021 (0.0060)	0.0035 (0.0039)	0.0036 (0.0043)	0.0098** (0.0044)	-0.0052 (0.0058)
Employment per capita by industry						
- Construction	0.00065 (0.00128)	0.00084 (0.00228)	0.00033 (0.00112)	-0.00005 (0.00164)	0.00042 (0.00143)	-0.00063 (0.00146)
- Manufacturing	0.00118 (0.00347)	-0.00037 (0.00391)	0.00318* (0.00167)	0.00293 (0.00306)	0.00390** (0.00186)	-0.0252** (0.0116)
- Retail	0.00034 (0.00089)	-0.00069 (0.00084)	0.00026 (0.00054)	-0.00014 (0.00062)	0.00022 (0.00063)	-0.00042 (0.00078)
- Health care and social services	-0.00002 (0.00058)	-0.00076 (0.00188)	-0.00012 (0.00054)	-0.00018 (0.00065)	0.00048 (0.00053)	0.00269* (0.00162)
- Farming	-0.00030 (0.00025)	-0.00031 (0.00021)	-0.00018 (0.00016)	-0.00017 (0.00013)	0.00006 (0.00017)	-0.00039** (0.00020)
- Government	0.00111 (0.00097)	0.00130 (0.00091)	0.00020 (0.00063)	0.00092 (0.00078)	0.00085* (0.00044)	0.00051 (0.00046)
Transfer payments by type						
- Retirement and disability	-17.7 (17.4)	-17.1 (14.7)	7.2 (9.3)	-8.4 (15.4)	-1.5 (12.3)	-1.7 (14.3)
- Medical	-7.4 (40.6)	11.3 (38.9)	34.6 (36.2)	-2.4 (35.7)	73.7** (33.2)	-43.6 (35.7)
- Medicare	0.6 (8.0)	6.7 (7.5)	6.6 (5.5)	1.7 (5.5)	16.2*** (6.3)	1.9 (7.0)
- Medicaid/SCHIP/other state programs	-3.9 (45.4)	8.1 (35.7)	31.4 (35.7)	0.2 (35.8)	59.0* (31.1)	-43.3 (31.8)
- Income maintenance	16.3* (9.8)	13.1** (6.4)	23.9*** (5.3)	19.9*** (7.1)	27.9*** (5.8)	17.6*** (5.3)
- Supplemental security income	1.7 (3.3)	-1.4 (2.6)	4.3** (2.1)	3.3 (2.4)	6.3*** (2.1)	0.9 (2.4)

Dependent Variable	PSM-NN With replacement		PSM-NN Without replacement	PSM-KM With replacement	MM-NN With replacement	
	Not bias corrected	Bias corrected	Not bias corrected	Not bias corrected	Not bias corrected	Bias corrected
- Family assistance	11.3*** (3.6)	12.6*** (4.1)	12.1*** (2.6)	9.5*** (2.7)	9.1*** (2.2)	11.6*** (3.6)
- Food stamps	2.2 (2.7)	1.4 (3.0)	4.3* (2.3)	3.5 (2.3)	7.0*** (2.4)	4.8* (2.6)
- Unemployment insurance	-17.7 (13.1)	14.0 (18.1)	-14.7* (7.9)	-12.4 (8.4)	-17.1* (8.8)	-1.9 (10.6)
- Veterans benefits	-1.5 (2.7)	-2.6 (2.6)	-1.6 (1.9)	-2.4 (2.6)	-0.4 (2.4)	0.3 (3.5)
- Federal education and training assistance	-3.7 (9.5)	9.1 (8.6)	-1.4 (4.8)	-1.3 (6.2)	-2.5 (6.3)	-6.8 (6.9)
Population	-95.2 (129.5)	-46.6 (141.7)	-205.2*** (74.9)	-132.2 (88.9)	-260.9*** (79.8)	-76.0 (92.0)

*, **, *** Difference statistically significant at 10%, 5%, and 1% levels, respectively.

Table 4. Mean changes in trends, 2002 to 2007 minus 2000 to 2001 or 2002, DRA minus matching counties (DDD estimator)[29]

Dependent Variable	**PSM-NN With replacement**		**PSM-NN Without replacement**	**PSM-KM With replacement**	**MM-NN With replacement**	
	Not bias corrected	**Bias corrected**	**Not bias corrected**	**Not bias corrected**	**Not bias corrected**	**Bias corrected**
Personal income per capita	119.8 (117.0)	-1.7 (141.9)	10.2 (86.0)	75.8 (98.8)	-153.2 (107.7)	150.6 (171.1)
Major components of personal income						
- Net earnings per capita	13.4 (90.6)	-65.3 (127.5)	-26.4 (82.9)	-13.8 (69.3)	-163.7* (92.4)	23.9 (130.4)
- Wages and salaries per capita	-9.6 (87.9)	-18.9 (107.1)	-35.0 (74.7)	-9.1 (72.1)	-51.0 (81.2)	80.1 (96.3)
- Dividends, interest and rent per capita	62.0* (34.9)	59.6 (36.5)	29.8 (34.4)	50.0 (37.9)	7.9 (37.8)	36.5 (58.5)
- Personal transfer payments per capita	44.4 (37.3)	3.9 (35.3)	6.8 (23.7)	40.1 (33.7)	2.5 (22.6)	90.0*** (32.6)
Earnings per capita by industry						
- Construction	-65.2 (132.8)	-143.0 (202.3)	-33.7 (107.2)	-48.1 (85.0)	-40.1 (102.3)	56.4 (110.7)
- Manufacturing	-33.2 (104.0)	-24.7 (143.7)	-112.1 (70.5)	-128.6 (118.8)	-133.7* (72.3)	-81.0 (108.0)
- Retail	-5.2 (18.0)	14.5 (16.4)	4.5 (15.2)	-2.9 (16.1)	2.0 (14.6)	29.6 (24.8)
- Health care and social services	-2.7 (30.8)	-2.1 (84.5)	4.5 (25.1)	-4.9 (39.7)	-21.4 (21.5)	-37.7 (47.7)
- Farming	35.7 (74.4)	-120.1 (93.3)	41.4 (56.0)	26.4 (69.2)	-158.8** (80.5)	-14.9 (107.8)
- Government	-44.4 (40.3)	-71.0** (32.3)	-12.0 (17.2)	-42.3* (24.9)	-36.4** (17.8)	-22.2 (18.4)

[29] Changes from 2001 to 2002 are subtracted for earnings and employment per capita by industry; all other subtracted changes are from 2000 to 2002.

Dependent Variable	PSM-NN With replacement		PSM-NN Without replacement	PSM-KM With replacement	MM-NN With replacement	
	Not bias corrected	Bias corrected	Not bias corrected	Not bias corrected	Not bias corrected	Bias corrected
Employment per capita	-0.00221 (0.00342)	-0.00225 (0.00339)	-0.00221 (0.00204)	-0.00234 (0.00243)	-0.00448 (0.00276)	0.00185 (0.00332)
Employment per capita by industry						
- Construction	-0.00113 (0.00198)	-0.00244 (0.00358)	-0.00029 (0.00154)	-0.00066 (0.00183)	-0.00033 (0.00180)	0.00147 (0.00200)
- Manufacturing	0.00040 (0.00407)	-0.00022 (0.00489)	-0.00314* (0.00182)	-0.00279 (0.00305)	-0.00443** (0.00188)	0.01051 (0.00720)
- Retail	-0.00025 (0.00086)	0.00039 (0.00093)	-0.00031 (0.00070)	-0.00042 (0.00062)	-0.00026 (0.00070)	0.00041 (0.00095)
- Health care and social services	-0.00006 (0.00134)	0.00066 (0.00339)	-0.00004 (0.00095)	-0.00012 (0.00095)	-0.00097 (0.00071)	-0.00250 (0.00171)
- Farming	0.00036 (0.00023)	0.00043* (0.00022)	0.00031 (0.00021)	0.00022 (0.00021)	0.00010 (0.00021)	0.00057** (0.00024)
- Government	-0.00132 (0.00099)	-0.00151 (0.00096)	-0.00040 (0.00062)	-0.00112 (0.00089)	-0.00101** (0.00051)	-0.00070 (0.00051)
Transfer payments by type						
- Retirement and disability	10.7 (7.5)	13.7* (7.6)	0.4 (5.5)	7.5 (7.0)	8.5 (6.4)	16.5* (9.9)
- Medical	22.3 (32.1)	8.3 (26.4)	4.9 (25.8)	25.3 (26.9)	-13.6 (20.8)	73.5*** (26.6)
- Medicare	7.8* (4.6)	8.5** (4.2)	8.0*** (3.1)	9.1*** (3.0)	7.1** (3.3)	7.8* (4.1)
- Medicaid/SCHIP/other state programs	12.2 (30.3)	-2.0 (26.4)	-4.5 (21.9)	14.1 (27.7)	-21.1 (20.2)	64.4** (25.5)
- Income maintenance	-0.6 (5.9)	-1.8 (5.2)	-6.8** (2.7)	-2.9 (4.8)	-4.6 (2.8)	-5.2 (3.3)
- Supplemental security income	0.5 (1.9)	2.1 (1.8)	-1.1 (1.0)	-0.7 (1.5)	-1.4 (1.3)	1.9 (1.7)

Dependent Variable	**PSM-NN With replacement**		**PSM-NN Without replacement**	**PSM-KM With replacement**	**MM-NN With replacement**	
	Not bias corrected	**Bias corrected**	**Not bias corrected**	**Not bias corrected**	**Not bias corrected**	**Bias corrected**
- Family assistance	-5.8** (2.4)	-7.1*** (2.5)	-6.6*** (1.4)	-5.2** (2.1)	-5.4*** (1.5)	-5.5*** (2.1)
- Food stamps	1.6 (1.7)	1.6 (2.0)	0.5 (1.0)	1.0 (1.5)	-0.8 (1.4)	-0.9 (1.5)
- Unemployment insurance	10.3 (10.3)	-13.8 (13.6)	7.9* (4.1)	7.5 (5.4)	11.4* (5.9)	-0.9 (6.9)
- Veterans benefits	1.8 (1.8)	2.7 (1.8)	2.0 (1.4)	3.1** (1.5)	3.0 (1.9)	2.4 (2.5)
- Federal education and training assistance	2.9 (4.4)	-2.8 (3.4)	0.9 (2.9)	1.1 (2.8)	1.4 (3.2)	3.0 (3.8)
Population	44.5 (58.9)	45.1 (73.1)	115.2*** (39.5)	42.7 (54.4)	151.3*** (57.3)	-42.1 (86.0)

*, **, *** Difference statistically significant at 10%, 5%, and 1% levels, respectively.

Table 5. Average treatment effects based on switching regressions for changes in outcomes, 2002 to 2007, DRA and matching counties (using PSM-NN without replacement) (N=206, except where noted)

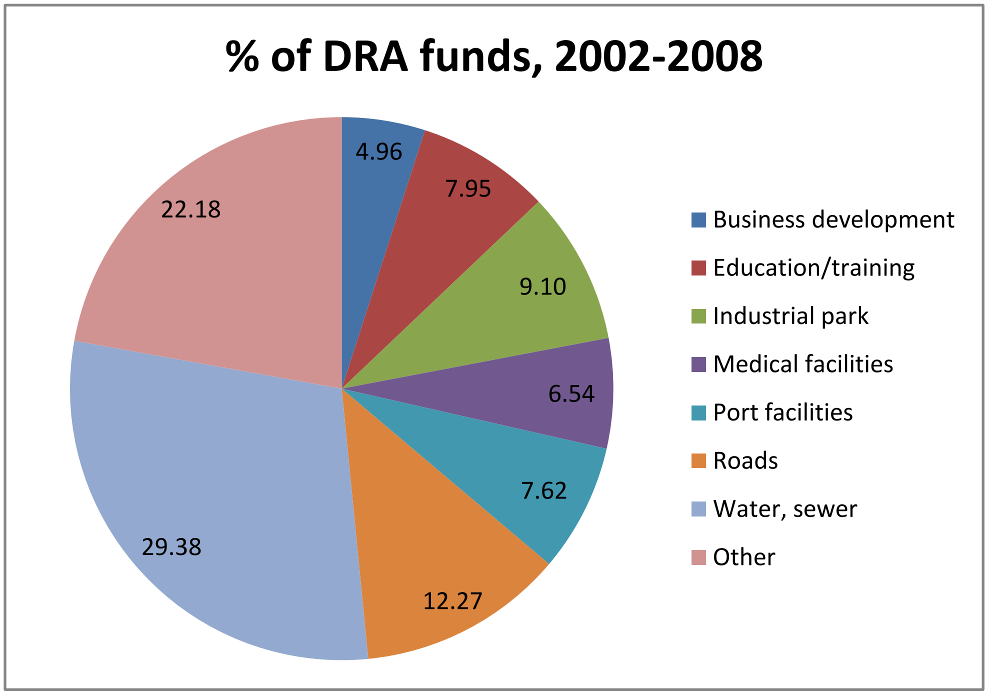
Dependent variable	**Regression 1**	**Regression 2**	
	DRA recipient	**DRA recipient**	**DRA funds per cap.**
Personal income per capita	518.2*** (176.9)	227.3 (211.9)	15.32** (6.34)
Major components of personal income			
- Net earnings per capita	226.0* (130.5)	76.4 (154.7)	7.88* (4.44)
- Wages and salaries per capita	85.8 (142.4)	-136.8 (171.5)	11.73** (5.17)
- Dividends, interest and rent per capita	123.7 (81.8)	79.6 (105.5)	2.32 (3.52)
- Personal transfer payments per capita	168.5*** (46.1)	71.3 (51.7)	5.12*** (1.34)
Earnings per capita by industry			
- Construction (N=149)	-70.5 (68.3)	-107.6 (86.3)	2.10 (2.99)
- Manufacturing (N=152)	98.4 (133.5)	50.7 (152.6)	2.69 (4.17)
- Retail (N=186)	3.4 (24.8)	9.2 (27.0)	-0.33 (0.60)
- Health care and social services (N=52)[30]	107.3 (98.6)	-16.3 (92.0)	8.21** (3.89)
- Farming	97.3* (54.4)	107.2* (64.0)	-0.52 (1.78)
- Government	-145.9** (67.7)	-178.6** (72.0)	1.72 (1.30)
Employment per capita	-0.0035 (0.0037)	-0.0037 (0.0045)	0.00001 (0.00013)
Employment per capita by industry			
- Construction (N=149)	-0.0012 (0.0013)	-0.0013 (0.0017)	0.00001 (0.00006)
- Manufacturing (N=152)	-0.0015 (0.0027)	-0.0013 (0.0030)	-0.00001 (0.00008)
- Retail (N=186)	-0.0009 (0.0008)	-0.0004 (0.0009)	-0.00003 (0.00002)
- Health care and social services (N=52)	-0.0008 (0.0019)	-0.0022 (0.0022)	0.00009 (0.00008)
- Farming	0.0001 (0.0002)	-0.0002 (0.0002)	0.000012** (0.000006)

[30] Regressions for earnings in health care and social services assume equal coefficients of covariates in treated and control samples with an intercept shift; full switching regressions were not possible due to the small number of observations.

Dependent variable	Regression 1	Regression 2	
	DRA recipient	DRA recipient	DRA funds per cap.
- Government	-0.0009 (0.0009)	-0.0005 (0.0011)	-0.000021 (0.000037)
Transfer payments by type			
- Retirement and disability	32.5** (15.6)	0.8 (18.0)	1.67*** (0.50)
- Medical	116.5*** (36.4)	69.2* (40.9)	2.49** (1.02)
- Medicare	45.6*** (13.0)	41.3*** (15.0)	0.23 (0.40)
- Medicaid/SCHIP/other state programs	72.3** (33.3)	29.2 (37.4)	2.27** (0.93)
- Income maintenance	7.1 (7.9)	0.2 (9.5)	0.36 (0.28)
- Supplemental security income	1.7 (3.3)	3.1 (4.0)	-0.08 (0.11)
- Family assistance (N=200)	-1.5 (1.6)	-5.0*** (1.9)	0.19*** (0.06)
- Food stamps	11.0*** (2.8)	11.8*** (3.6)	-0.04 (0.12)
- Unemployment insurance	-2.4 (5.4)	-4.0 (5.9)	0.08 (0.13)
- Veterans benefits	9.3** (4.7)	2.9 (5.3)	0.34** (0.13)
- Federal education and training assistance (N=194)	1.4 (5.6)	-0.8 (6.3)	0.14 (0.17)
Population	-363.0* (187.2)	-383.3** (195.8)	1.071 (3.020)
Share of population over 65 years of age	-0.00018 (0.00092)	-0.00090 (0.00109)	0.000038 (0.000031)
African American share of population	0.00163* (0.00096)	0.00318*** (0.00110)	-0.000082*** (0.000029)
Number of non-federal medical doctors per capita	0.000027 (0.000023)	0.000012 (0.000027)	0.0000008 (0.0000008)
Number of registered nurses (FTE) per capita	-0.000144 (0.000132)	-0.000118 (0.000158)	-0.0000014 (0.0000045)
Number of hospital beds per capita	-0.00059** (0.00024)	-0.00129*** (0.00031)	0.000037*** (0.000011)

*, **, *** Coefficient statistically significant at 10%, 5%, and 1% levels, respectively.

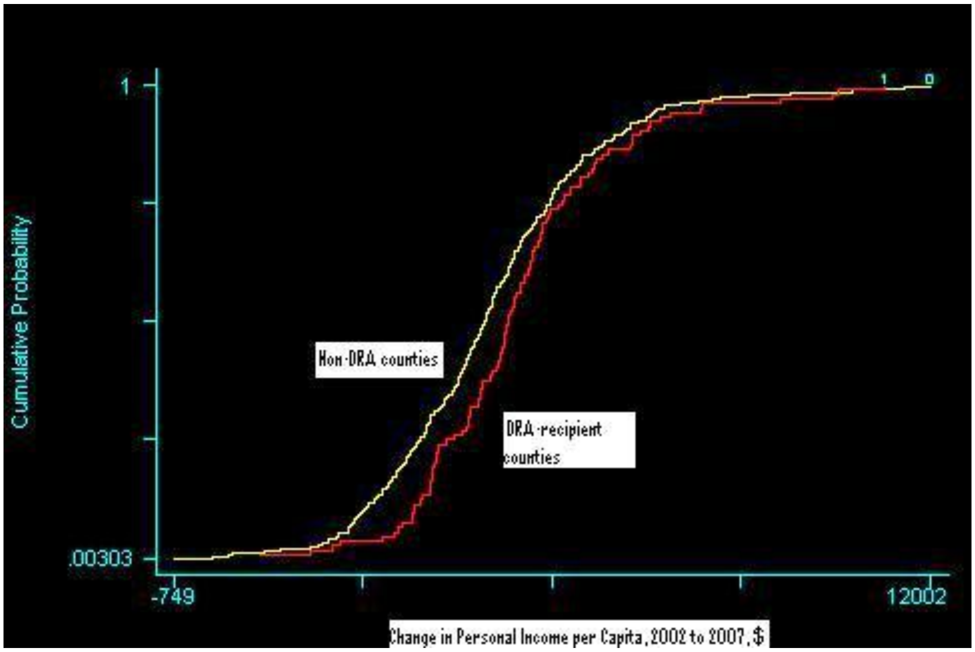
Figure 1. Allocation of DRA project funds, 2002-2008



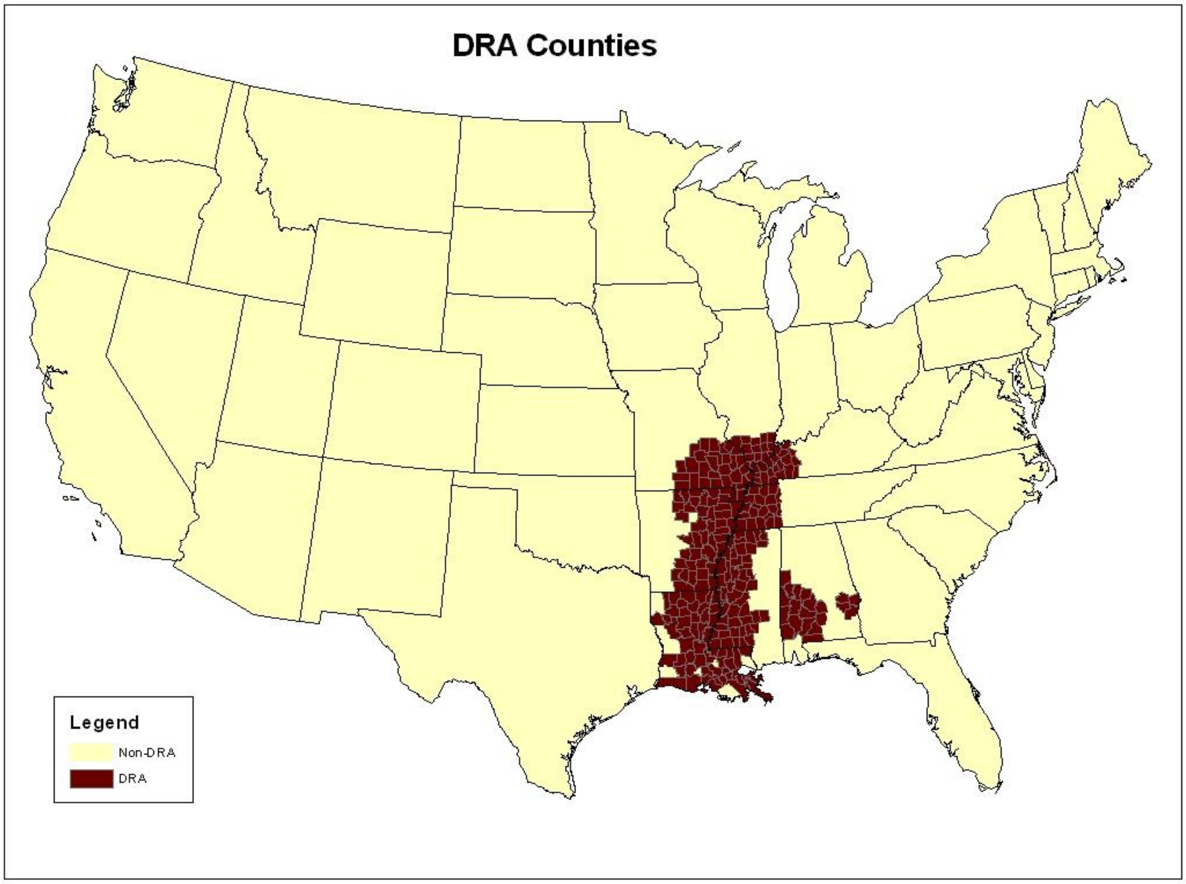
Source: Calculated from DRA (2009)

Figure 2. Cumulative density functions of change in personal income per capita, 2002 to 2007

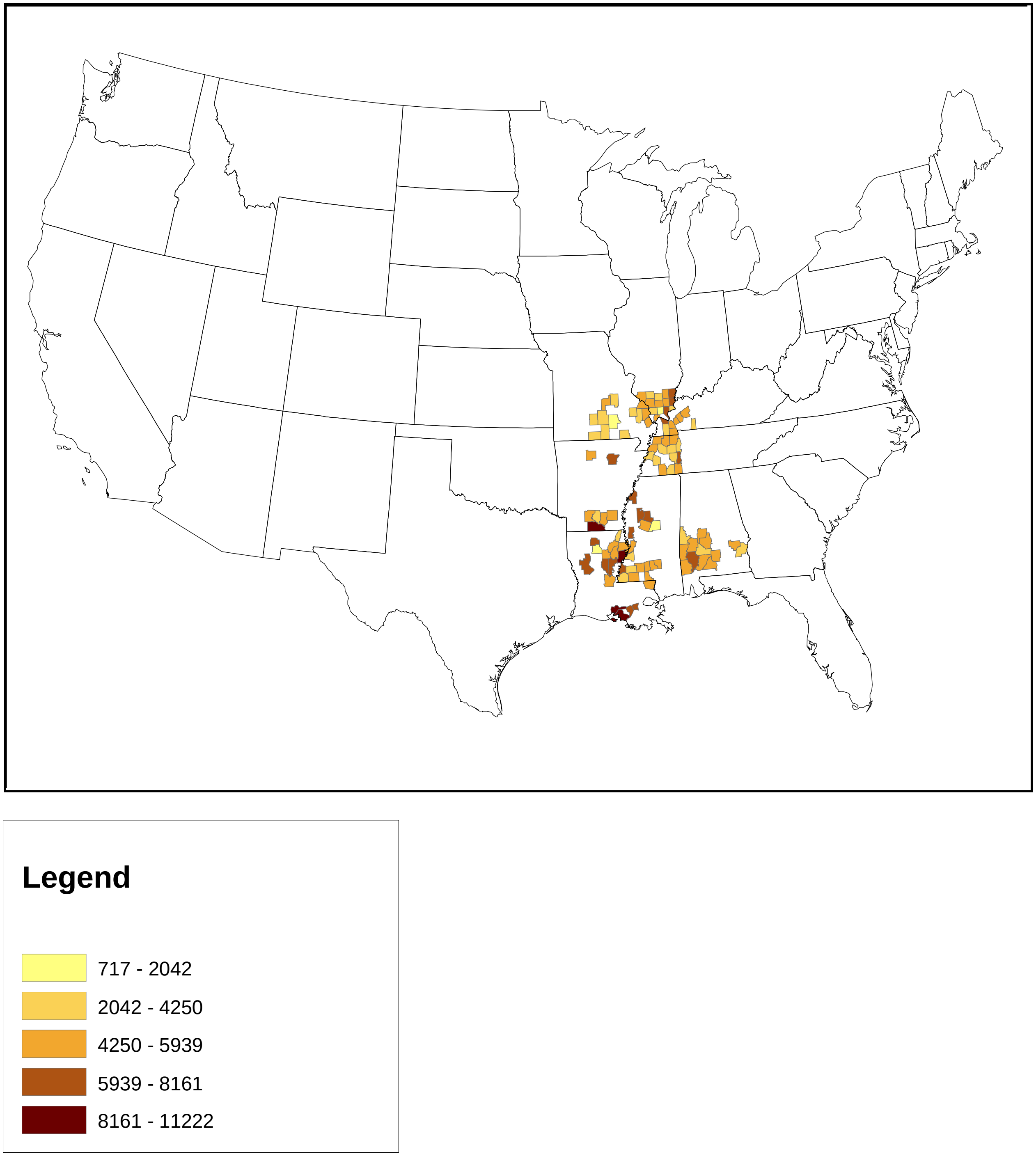
Matched DRA-recipient counties and non-DRA counties, using PSM-NN without replacement



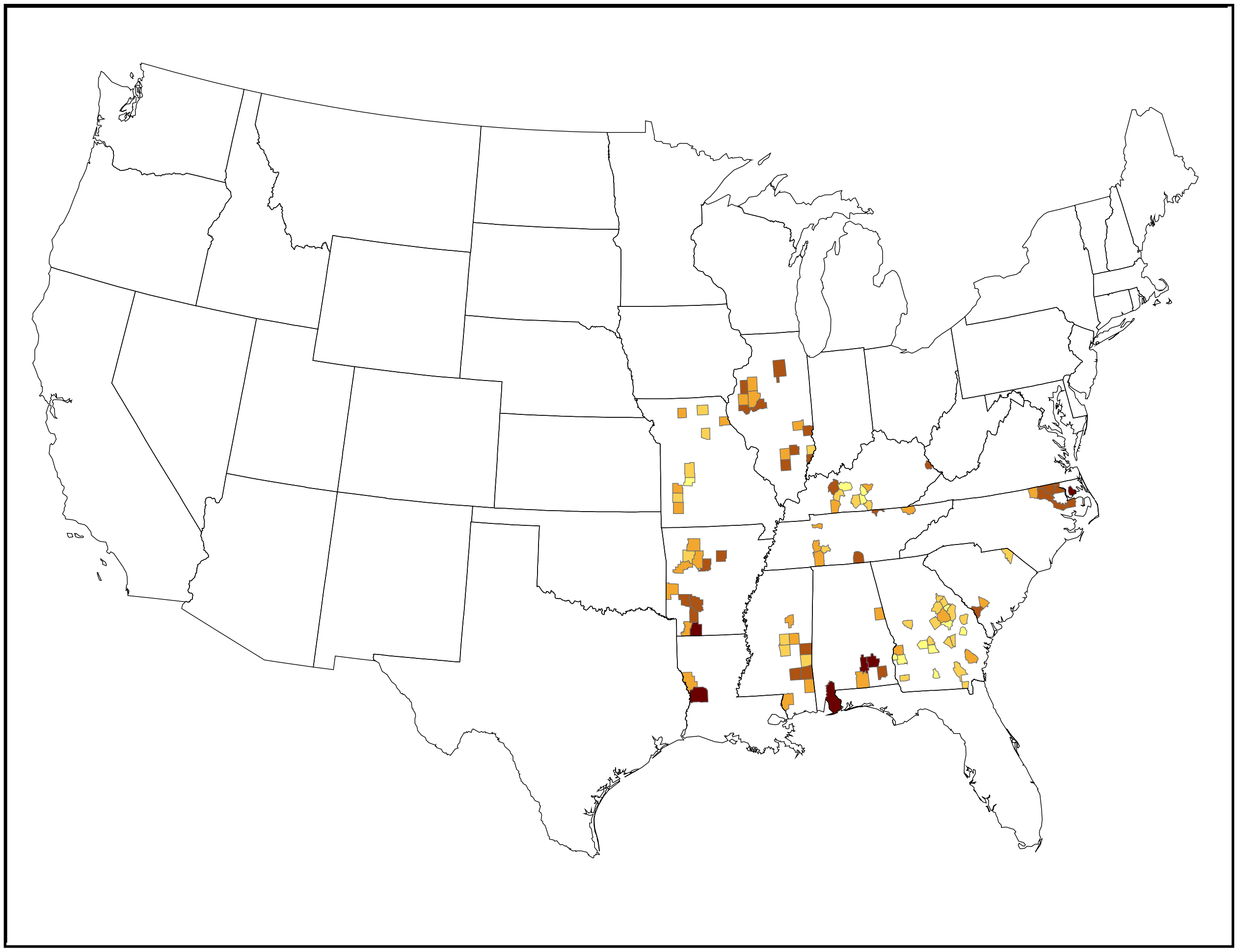
Map 1. DRA – eligible counties



Map 2. Change in personal income per capita in matched non-metro DRA-recipient counties, 2002-2007



Map 3. Change in personal income per capita in matched non-metro non-DRA counties, 2002-2007 (matched using PSM-NN without replacement)



Legend

215 - 2454
2454 - 3814
3814 - 5049
5049 - 6748
6748 - 11398

Annex Tables.

Table A1. Probit model to estimate propensity scores[a]

Explanatory variable	Coefficient	Std. Err.	P>z
Personal income per capita, 2000 ($)	-6.42E-05	6.86E-05	0.349
Population, 2000	-2.83E-06	7.46E-06	0.705
Poverty rate, 2000 (%)	0.0793	0.0523	0.130
Share of personal income from personal transfer payments, 2001	-4.812	4.160	0.247
Share of personal income from dividends, interest and rent, 2001	-11.516	4.253	0.007***
Share of adults employed in agriculture, forestry, fishing or hunting, 2000	-16.331	5.577	0.003***
Share of adults employed in construction, 2000	-9.235	5.355	0.085*
Share of adults employed in manufacturing, 2000	-0.081	2.393	0.973
Share of adults employed in retail trade, 2000	-4.090	6.531	0.531
Share of adults employed in public administration, 2000	-5.696	5.637	0.312
Share of adults employed in educational services, 2000	-8.233	4.959	0.097*
Share of adults employed in health care or social services, 2000	8.406	4.734	0.076*
Federal economic development grant funds per capita, 2000-01	0.000234	0.000177	0.188
Gulf Opportunity Zone counties	1.018	0.320	0.001***
Cotton harvested acres per capita, 2002	0.249	0.102	0.014**
Rice harvested acres per capita, 2002	4.216	1.378	0.002***
Distance to nearest urban center of 25,000 or more, 1980	-0.003047	0.003676	0.407
Distance to nearest urban center of 100,000 or more, 1980	0.000397	0.001844	0.829
Distance to nearest urban center of 250,000 or more, 1980	0.001372	0.001222	0.262
Distance to nearest urban center of 500,000 or more, 1980	-0.000379	0.000844	0.654
Distance to nearest urban center of 1,000,000 or more, 1980	-0.000556	0.000511	0.277
Population density, 1990 (persons/sq. mile)	-0.000158	0.004383	0.971
Rural share of population, 2000	-0.9866	0.5895	0.094*
Farm share of population, 2000	0.7628	4.3035	0.859
Black share of population, 2000	-0.9836	0.8337	0.238
Share of population age 17 or less, 2000	-11.078	5.619	0.049**
Share of population age 65 or more, 2000	4.097	6.605	0.535
Share of adults with more than a high school education, 2000	-0.195	2.223	0.930
Share of men working full time all year, 2000	1.831	2.916	0.530
Share of women working full time all year, 2000	-18.584	3.572	0.000***

[a] Number of observations = 461. Pseudo R^2 = 0.4293.

*, **, *** Coefficient statistically significant at 10%, 5%, and 1% levels, respectively.

Table A2. Comparison of characteristics of unmatched and matched DRA and comparison samples, using PSM-NN matching with replacement

Variable	Sample	Mean		%bias	p>|t|
		Treated	Control		
Personal income per capita, 2000 ($)	Unmatched	18755	20703	-70.3	0.000***
	Matched	19147	19139	0.3	0.981
Population, 2000	Unmatched	23876	26265	-12.4	0.273
	Matched	24483	26102	-8.4	0.559
Poverty rate, 2000 (%)	Unmatched	19.96	15.63	81.8	0.000***
	Matched	19.02	18.20	15.6	0.212
Share of personal income from personal transfer payments, 2001	Unmatched	0.2681	0.2277	83.3	0.000***
	Matched	0.2616	0.2620	-0.9	0.951
Share of personal income from dividends, interest and rent, 2001	Unmatched	0.1659	0.1839	-51.9	0.000***
	Matched	0.1702	0.1674	8.2	0.458
Share of adults employed in agriculture, forestry, fishing or hunting, 2000	Unmatched	0.0568	0.0540	7.8	0.446
	Matched	0.0484	0.0481	0.8	0.943
Share of adults employed in construction, 2000	Unmatched	0.0720	0.0775	-26.7	0.011**
	Matched	0.0750	0.0726	11.5	0.408
Share of adults employed in manufacturing, 2000	Unmatched	0.1978	0.2225	-33.0	0.001***
	Matched	0.1990	0.2180	-25.4	0.069*
Share of adults employed in retail trade, 2000	Unmatched	0.1141	0.1136	3.3	0.757
	Matched	0.1149	0.1152	-1.4	0.925
Share of adults employed in public administration, 2000	Unmatched	0.0559	0.0527	13.4	0.200
	Matched	0.0542	0.0496	19.3	0.120
Share of adults employed in educational services, 2000	Unmatched	0.0926	0.0835	33.2	0.001***
	Matched	0.0927	0.0891	12.9	0.386
Share of adults employed in health care or social services, 2000	Unmatched	0.1156	0.1077	31.5	0.002***
	Matched	0.1167	0.1165	0.9	0.953
Federal economic development grant funds per capita, 2000-01 ($)	Unmatched	367.21	285.78	17.6	0.097*
	Matched	336.88	304.91	6.9	0.538
Gulf Opportunity Zone counties (share of counties)	Unmatched	0.1832	0.0394	46.8	0.000***
	Matched	0.2233	0.3010	-25.3	0.207
Cotton harvested acres per capita, 2002	Unmatched	0.9494	0.3330	43.1	0.000***
	Matched	0.5409	0.2451	20.7	0.079*
Rice harvested acres per capita, 2002	Unmatched	0.5834	0.0017	57.8	0.000***
	Matched	0.0430	0.0282	1.5	0.395
Distance to the nearest urban center of 25,000 or more, 1980 (miles)	Unmatched	37.28	35.03	8.4	0.424
	Matched	37.04	38.22	-4.4	0.737
Distance to the nearest urban center of	Unmatched	85.43	82.53	5.3	0.611

Variable	Sample	Mean Treated	Mean Control	%bias	p>|t|
100,000 or more, 1980 (miles)	Matched	85.72	86.25	-1.0	0.943
Distance to the nearest urban center of 250,000 or more, 1980 (miles)	Unmatched	149.04	139.37	11.1	0.290
	Matched	146.89	157.16	-11.8	0.380
Distance to the nearest urban center of 500,000 or more, 1980 (miles)	Unmatched	236.17	225.97	7.1	0.503
	Matched	235.90	244.16	-5.8	0.682
Distance to the nearest urban center of 1,000,000 or more, 1980 (miles)	Unmatched	377.79	397.67	-9.9	0.358
	Matched	371.46	395.03	-11.8	0.398
Population density, 1990 (persons/square mile)	Unmatched	40.29	46.06	-17.3	0.103
	Matched	42.61	41.19	4.3	0.738
Rural share of population, 2000	Unmatched	0.6785	0.7066	-12.3	0.222
	Matched	0.7070	0.7452	-16.7	0.256
Farm share of population, 2000	Unmatched	0.0317	0.0501	-54.1	0.000***
	Matched	0.0341	0.0381	-11.7	0.296
Black share of population, 2000	Unmatched	0.2805	0.1868	43.3	0.000***
	Matched	0.2590	0.2524	3.1	0.828
Share of population age 17 or less, 2000	Unmatched	0.2583	0.2505	29.3	0.002***
	Matched	0.2546	0.2511	13.0	0.345
Share of population age 65 or more, 2000	Unmatched	0.1495	0.1520	-9.2	0.398
	Matched	0.1504	0.1577	-27.4	0.053*
Share of adults with more than a high school education, 2000	Unmatched	0.3269	0.3490	-30.6	0.004***
	Matched	0.3370	0.3301	9.5	0.508
Share of men working full time all year, 2000	Unmatched	0.5732	0.6144	-71.4	0.000***
	Matched	0.5764	0.5778	-2.5	0.860
Share of women working full time all year, 2000	Unmatched	0.3937	0.4308	-102.7	0.000***
	Matched	0.3915	0.3912	0.8	0.957
Overall balance tests			Pseudo R^2	LR chi^2	$p>chi^2$
	Unmatched		0.429	236.26	0.000***
	Matched		0.140	40.12	0.103

*, **, *** Difference statistically significant at 10%, 5%, and 1% levels, respectively.

Table A3. Comparison of characteristics of matched DRA and comparison samples, using PSM-NN matching without replacement, PSM-KM and Mahalanobis nearest neighbor matching

Variable	Mean Treated	PSM-NN without replacement			PSM-KM			Mahalanobis		
		Control	%bias	p>|t|	Control	%bias	p>|t|	Control	%bias	p>|t|
Personal income per capita, 2000 ($)	19147	19385	-8.6	0.512	19205	-2.1	0.876	19865	-25.9	0.036**
Population, 2000	24483	23345	5.9	0.655	26026	-8.0	0.542	28348	-20.0	0.069*
Poverty rate, 2000 (%)	19.02	17.56	27.6	0.035**	18.07	18.0	0.160	16.46	48.3	0.000***
Share of personal income from personal transfer payments, 2001	0.2616	0.2536	16.5	0.250	0.2585	6.5	0.650	0.2372	50.3	0.000***
Share of personal income from dividends, interest and rent, 2001	0.1702	0.1708	-1.7	0.884	0.1673	8.4	0.473	0.1683	5.5	0.629
Share of adults employed in agriculture, forestry, fishing or hunting, 2000	0.0484	0.0496	-3.4	0.757	0.0456	7.9	0.462	0.0464	5.6	0.589
Share of adults employed in construction, 2000	0.0750	0.0752	-1.1	0.937	0.0763	-6.2	0.657	0.0762	-6.2	0.628
Share of adults employed in manufacturing, 2000	0.1990	0.2202	-28.3	0.040**	0.2113	-16.4	0.235	0.2388	-53.1	0.000***
Share of adults employed in retail trade, 2000	0.1149	0.1134	9.2	0.523	0.1139	6.3	0.671	0.1145	2.3	0.843
Share of adults employed in public administration, 2000	0.0542	0.0518	10.4	0.433	0.0523	8.3	0.523	0.0460	34.4	0.004***
Share of adults employed in educational services, 2000	0.0927	0.0901	9.5	0.557	0.0888	14.1	0.329	0.0865	22.2	0.140
Share of adults employed in health care or social services, 2000	0.1167	0.1139	11.4	0.414	0.1171	-1.6	0.911	0.1058	43.6	0.001***
Federal economic development grant funds per capita, 2000-01 ($)	336.88	298.61	8.3	0.479	281.99	11.9	0.313	287.84	10.6	0.275
Gulf Opportunity Zone counties	0.2233	0.1262	31.6	0.067*	0.2976	-24.2	0.226	0.2039	6.3	0.735
Cotton harvested acres per capita, 2002	0.5409	0.3899	10.5	0.406	0.3181	15.6	0.197	0.2558	19.9	0.116
Rice harvested acres per capita, 2002	0.0430	0.0048	3.8	0.007***	0.0239	1.9	0.257	0.0236	1.9	0.249
Distance to the nearest urban center of 25,000 or more, 1980 (miles)	37.04	34.62	9.0	0.502	36.98	0.2	0.986	35.84	4.4	0.730

Variable	Mean Treated	PSM-NN without replacement			PSM-KM			Mahalanobis		
		Control	%bias	p>|t|	Control	%bias	p>|t|	Control	%bias	p>|t|
Distance to the nearest urban center of 100,000 or more, 1980 (miles)	85.72	76.31	17.2	0.209	87.50	-3.3	0.814	84.62	2.0	0.878
Distance to the nearest urban center of 250,000 or more, 1980 (miles)	146.89	147.20	-0.4	0.980	154.10	-8.3	0.557	136.83	11.6	0.361
Distance to the nearest urban center of 500,000 or more, 1980 (miles)	235.90	220.83	10.5	0.447	238.68	-1.9	0.892	210.46	17.8	0.202
Distance to the nearest urban center of 1,000,000 or more, 1980 (miles)	371.46	362.64	4.4	0.745	383.39	-6.0	0.666	316.47	27.5	0.020**
Population density, 1990 (persons/sq. mile)	42.61	38.48	12.4	0.295	41.83	2.4	0.854	43.48	-2.6	0.838
Rural share of population, 2000	0.7070	0.7187	-5.2	0.721	0.7372	-13.2	0.356	0.6974	4.2	0.753
Farm share of population, 2000	0.0341	0.0416	-22.1	0.047**	0.0336	1.5	0.882	0.0399	-17.0	0.067*
Black share of population, 2000	0.2590	0.2118	21.9	0.132	0.2301	13.4	0.338	0.1995	27.5	0.039**
Share of population age 17 or less, 2000	0.2546	0.2484	23.5	0.097*	0.2523	8.5	0.547	0.2525	7.7	0.584
Share of population age 65 or more, 2000	0.1504	0.1549	-16.9	0.210	0.1531	-10.0	0.464	0.1485	7.4	0.524
Share of adults with more than a high school education, 2000	0.3370	0.3343	3.7	0.798	0.3383	-1.8	0.897	0.3389	-2.8	0.836
Share of men working full time all year, 2000	0.5764	0.5870	-18.3	0.198	0.5827	-10.9	0.444	0.6112	-60.3	0.000***
Share of women working full time all year, 2000	0.3915	0.4044	-35.8	0.010***	0.3922	-2.1	0.886	0.4137	-61.7	0.000***
		Pseudo R^2	LR chi^2	$p>chi^2$	Pseudo R^2	LR chi^2	$p>chi^2$	Pseudo R^2	LR chi^2	$p>chi^2$
Overall balance tests – matched samples		0.097	27.65	0.589	0.047	13.33	0.996	0.218	62.29	0.000***

*, **, *** Difference statistically significant at 10%, 5%, and 1% levels, respectively.